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ESSAYS BY

LANCELOT HOGBEN
M.A., D.SC., F.R.S.

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1

THE RACE CONCEPT

A DARWIN CENTENARY ADDRESS DELIVERED AT A SYMPOSIUM OF THE INSTITUTE OF RACE RELATIONS

In any sense in which the term *race* is intelligible to a biologist, it signifies a group which preserves more or less clear-cut characteristics within the framework of a wider assemblage of interfertile forms as the outcome of mutation pressure or selection in circumstances of geographical isolation or inbreeding imposed by human interference. All the available evidence about so mobile a species as *Homo sapiens* points to a long history of migrations in which very few assemblages of genotypes have remained in isolation long enough to ensure a high measure of homogeneity; and nothing we have learned during the past century about the geographical distribution of human physique has any certain relevance to the diversity of Man's unequally distributed cultural achievements. That a concept which has accordingly so exiguous a bearing on the contemporary distribution of the species has been the focus of so much unprofitable controversy would indeed be an enigma, if the peculiar circumstances incidental to the revival of evolutionary speculation in the mid-nineteenth century gave us no clue to the intellectual preoccupations of a period during which it emerged into prominence after the publication of Darwin's *Origin of Species*.

In recent times, physique has equipped few men more felicitously than Charles Darwin and Karl Marx to fulfil the role of father figure. Devoted followers of each have fathered on them views which they could not possibly have entertained in their lifetimes unless endowed with second sight. Such has been the destiny of many others, notably Newton; but judicious appraisal of Newton's contributions

in his own generation did not arouse passions which a discussion of the views of Marx still excites, and those of Darwin, at least till lately, excited. Happily, one can now re-examine Darwin's credentials before an audience that attained intellectual maturity after the controversy over his teaching had begun to cool off.

To get the major issues sharply into focus, certain facts about the material circumstances and mental climate of Darwin's time are highly relevant. Of the former, three are most significant. During the thirty years before *The Origin of Species* appeared:

(a) oceanic navigation under steam power had both greatly enlarged opportunities for a global survey of living beings and forced the problems of geographical distribution on the attention of naturalists;
(b) surveying of the type initiated during the period of canal construction had received a new impetus from railway construction;
(c) optical technology had borne fruit in cardinal improvements both of the microscope and of the telescope.

The first of these signalized the acquisition of a heretofore undiscussed body of data challenging, if only because of their novelty, to traditional views concerning the origin of the contemporary diversity of animal and plant life. The second had equipped the collector of fossils with contour maps confirmatory of the general picture of the formation of the earth's crust first advanced by Hutton in the context of a precocious outburst of evolutionary speculation to which Erasmus, the grandfather of Charles Darwin, had contributed. Before 1810, there was little or no basis for anticipation of rational grounds for presuming an orderly succession of new types throughout a period of time vastly greater than the duration of the written record of human existence. By 1860, the principle of geological succession was a commonplace.

In the same milieu, advances of optical technology are revealing in a different sense. In its own setting, the microscope which accompanied Darwin on the *Beagle* was not despicable, though by modern standards fitting only as a Christmas gift for an eleven-year plus. Darwin himself was about twenty years old when the newer microscopes bore fruit in Amici's discovery that one pollen grain fertilizes one ovule, and when Cuvier assigned the human sperm as a parasitic

organism to the genus Cercaria in his *Règne Animal.* The reproductive processes of Cryptogams then justified their designation. That few zoologists in 1850 accepted the doctrine that one animal sperm fertilizes one ovum sufficiently explains why no zoologists—including Darwin and Wallace—recognized the wider implications of Mendel's work published nine years after the reading of their joint communication. Modern methods of fixation began to come into use during the fifties; but there were as yet no microtomes which could take advantage of the fact. The numerical constancy of the chromosomes was unrecognized. Indeed, Hertwig and Fol gave the first ocular demonstration that only one sperm fertilizes one ovum some fifteen years after *The Origin of Species* appeared; and the date is especially eloquent because any intelligible meaning we now attach to the term biparental inheritance in a biological context is referable to the material contribution of the sperm and the ovum to the outcome of the developmental process.

The three material features of the setting for the revival of evolutionary speculation at the end of the sixth decade of the nineteenth century have special interest if we also recall that it happened at a time when three major controversies dominated the ideological scene:

(a) Well into Darwin's manhood, well-nigh all the English leaders of natural science, Faraday and Joule like Davy, Henry, Dalton and Priestley before them, being nonconformists, had no university training. The Godless College of Gower Street was non-existent in Darwin's boyhood, and a Test Act excluded nonconformists from Cambridge till ten years, Oxford till twenty years, after the *Origin* appeared. Till the latter date, and throughout nearly the whole of Darwin's lifetime, the claims of ecclesiastical and secular authorities to control the educational system at every level were the focus of bitter controversy.

(b) The repeal of the Corn Laws occurred twelve years before the celebrated joint communication, and throughout the greater period of Darwin's lifetime the notion of free competition as a social ethic was a focus of secular controversy debated with scarcely less vehemence than the right of the Bishops' bench to censor a curriculum of naturalistic studies.

(c) Darwin could recall the abolition of the British slave trade during his boyhood, and had started his professional career as a naturalist in the Royal Navy before the prohibition of slave ownership in the British Colonies. The abolition controversy was at its height during the decade which ended with the appearance of the *Origin*. The American Civil War began within four years of this event, and each side had vehement supporters in Britain.

With the end in view and with the space at my disposal, it is not possible to dovetail all these clues to the impact of the *Origin* on his contemporaries. So I shall say no more about the relevance of the *laissez faire* controversy to the peculiar appeal of the Malthusian jingle in the disillusion subsequent to the bright hopes of the French physiocrats. Nor shall I deal with the comic irrelevance of Herbert Spencer's beatification of natural selection as the Survival of the Fittest. None the less, we cannot judiciously assess the effect of Darwin's teaching on the discussion of racial questions, if we regard the issues involved as exclusively factual or logical. Biologists of the mid-nineteenth century approached them with social preoccupations intelligible only in the context of abolition and with views about the bearing of geographical varieties in general on the evolutionary problem intelligible only in the context of theological debate. Indeed a brief digression on the Test Act situation in its own technological setting will usefully emphasize how exiguous were the logical ties between what Darwin asserted and contemporary interpretation of his teaching, the more so because the *Origin* evoked no controversy conducted with comparable vehemence in contemporary France, where men of science in the tradition of the *Ecole Polytechnique* were mostly *libres penseurs*.

In its own social context, we have seen that technological advances had made possible considerable advances of scientific knowledge contributory to a formidable body of geological and geographical data which a Biblical view of creation, upholstered by Paley's natural theology with a teleological rationale, could not readily co-ordinate in a framework satisfactory to a sceptic. In the same milieu as the Test Act controversy, the untimely intervention of Bishop Wilberforce against so astute a champion of debate as Thomas Henry Huxley at the British Association of 1860 set what was to be the

pattern of a contest which proceeded for more than a generation within the framework of common acceptance of a stupendous *non sequitur*. Thenceforth, both contestants subscribed to the dual proposition: if Darwin and Wallace are right, Genesis is wrong, and if Genesis is wrong, Darwin and Wallace are right.

The first half of the proposition is unexceptionable, and the second is silly unless we concede that there are only two conceivable accounts of the origin of the diversity of living beings. It is therefore essential to remind ourselves that evolution is not the bare statement subsumed in the factual assertion embodied in the historic principle of geological succession. The doctrine of evolution makes the *additional* assertion that the divergent succession of living types is a consequence of the two circumstances incidental to the normal process of generation now subsumed by the terms hereditary transmission and genetic variation. In fact, as earlier remarks on the role of the microscope have sufficiently emphasized, it was not possible at that time to formulate clearly what we mean by the distinction and it was wholly impossible therefore to produce experimental evidence in support of the addendum.

Doubtless Darwin, who discreetly kept in the background of what was essentially an ideological controversy little to his taste, sensed this deficiency. At least, he followed up his masterly assemblage of facts invitatory to speculation on origins at a new level of discussion by publishing an anthology of domestic varieties of animals. For two reasons, we can say in retrospect without hesitation that *Animals and Plants under Domestication* adds nothing conclusive to the debate from a scientific, in contradistinction to an ideological, viewpoint. One is that most, if not all, of the data are susceptible to the interpretation that new domesticated varieties have arisen by recombination of highly diversified gene complexes through crossing different interfertile local varieties; and the possibility of change through such a process in the absence of mutation terminates automatically when we have extracted all viable recombinations. The other circumstance that deprives the argument of cogency depends on whether we define species in terms of what botanists sometimes respectively call Linnaean and Jordanian species. The latter include interfertile geographical varieties, but the former include units which either produce sterile offspring or no offspring at all when mated *inter se*. Darwin's data with respect to animals and plants under domestica-

tion throw no light whatever on how bisexual forms can arise, if inter-sterile with parent stock in this sense.

Only during the last half century have we gained conclusive proof of the mutation process and only in our generation have we obtained clear-cut evidence that new forms intersterile with parent stock can arise without supernatural intervention. Otherwise, as recognized by Philip (the father of Edmund) Gosse, himself a professional geologist, we are free to interpret the historical record of the rocks as an indefinitely protracted sequence of acts on the part of a suppositious creator. As a devout Plymouth Brother, Philip Gosse conceived the time scale of the sequence to be commensurate with the majesty of Jehovah with full scriptural authority for the assertion that a day is with the Lord as a thousand years. On this understanding, anyone with a flair for the esoteric use of language can accommodate the facts as then known with a poetic interpretation of the Pentateuch narrative.

Such vagueness of the species concept, as Darwin's zoological contemporaries used the term, casts a long shadow over the discussion of inter-fertile local varieties of *Homo sapiens* in the ideological context of the abolition controversy. Taxonomists had not—and have not yet—resolved the antinomy of natural and artificial classification at the level discussed so informatively and inconclusively by Whewell in the first half of the nineteenth century. The popularity of evolution exonerated them from further discussion of the issue in terms of the adequacy or inadequacy of a traditional two-valued logic by endowing the task with a new and, as we now see, unattainable objective which condemned zoology to wander for forty years in a barren wilderness of phylogenetic speculation. In the exhilarating climate of emancipation from Paley's natural theology, transitional types whose intrusion into an otherwise tidy taxonomy had hitherto been a liability, had now the assurance of a cordial welcome as missing links. In short, Darwin's followers regarded the best arrangement of species within genera as an arrangement which mirrors phyletic relationships. By the same token, they equally condoned the propriety of discussing which interfertile local varieties within a species are more or less ancestral. Fortified by the belief that human fatigue is the only obstacle to the elucidation of such pedigrees by recourse to anatomical data, taxonomists undertook their self-consecrated heraldic task with no misgivings about the outcome.

Indeed, it will be difficult to believe that such a hope could have sustained such stupendous persistence in fruitless and trivial exploits of repetitive mensuration, if we do not fortify our credulity with the reflection that persons of considerable intellectual standing enthusiastically subscribed to the cult of phrenology when craniometry was still in the cradle. Even so, early measurements should have sufficed to damp the ardour of the most credulous craniometrician, if rational considerations had any relevance to the issue. The cranial capacity of Bismarck was 1,965 c.c. and his brain weighed 1,867 grams. The cranial capacity of Leibnitz, who advanced mathematics as few others of a very creative period, anticipated the study of comparative linguistics and managed the financial affairs of the Elector who founded our own Hanoverian dynasty, was 1,422 c.c. and his estimated brainweight was 1,257 grams. The figure for cranial capacity is instructive placed side by side with the following means:

Buriats (Mongols)	1,496
Kaffirs	1,460
Eskimos	1,563
Amerindians	1,450
La Chapelle Man	1,620

How lately such considerations influenced discussion of the geographical distribution of human endowments is evident from an autopsy on an (at the time) widely quoted Carnegie Institution publication by Davenport and Steggerda (1930), based on a study of 372 Jamaicans chromatically classified by the authors as Blacks 105, Whites 100 and Browns 165. By that time the I.Q. had come into the field as a serious competitor to craniometrical precision. On the basis of samples not too sizeable to exorcize scrupulous attention to the method of social selection employed, the authors arrive (*inter alia*) both at: (a) the encouraging conclusion that Whites were 'outstandingly superior in their ability' to detect ridiculous conclusions, since they did best in Tests No. III (answer to 'common sense' questions), No. IV (meaning of words) and No. V (reconstruction of pied sentences); (b) the discomforting disclosure that the Blacks excelled in Tests No. I (following complicated directions), No. II (problems in mental arithmetic), No. VI (recognizing and continuing numerical series) and No. VI (logical relations and analogies). The authors adroitly talk their way out of this dilemma by asserting that 'the

Blacks seem to do better in simple arithmetic and with numerical series' because 'it seems a plausible hypothesis for which there is considerable support that the more complicated a brain, the more numerous its association fibres, the less satisfactorily it performs the simple numerical problems which a calculating machine does so quickly and accurately'. It would be paranoid to attribute a mischievous intention to any whose criteria of reasoning are so sub-standard.

Both contemporary genetical considerations referable to repetitive and back-mutation at the same locus and palaeontological evidence of the extent of convergence in the past, have now taught us to dismiss the possibility that we can ever hope to tailor the terminal twigs of a taxonomic system to a historical sequence; but the illusion that it is possible to do so was undoubtedly the midwife to the discipline we now call physical anthropology. Reference to the literature of the time abundantly discloses that one circumstance propitious to this was the hope of demonstrating that the African Negro is more primitive in a zoological sense than the Southern gentry and their supporters. Whence we are to conclude that the African is: (a) less teachable; (b) not entitled to use the ballot box; (c) a suitable beast of burden for the white man. Each of the last three statements is a *non sequitur*, though little recognized as such in the highly impassioned climate of debate at the time of the American Civil War and its aftermath. Today, it suffices to comment that the hope which sustained the undertaking is itself illusory.

A humane man, who expresses in his Journal profound disgust towards the institution of slave ownership, Darwin bears no responsibility for this perversion of his teaching. It is also relevant to recall that the *Origin* appeared before the publication of the microbiological researches of Koch and the immunological studies of Pasteur. Only during the last fifty years have parasitological investigations familiarized us with a host of data relevant to the question: what obstacles have retarded the technological development of the African peoples? Of itself, the fact that domesticable ungulates in general and the horse in particular are highly susceptible to trypanosomiasis has condemned Africans south of the Tropic of Cancer to be their own beasts of burden in territories where malaria,[1] yellow

[1] One often hears from those who should know better that natural selection has made the African immune to malaria. In fact malaria is a major menace of the African village in the early years of life.

fever, hookworm, bilharzia and many other diseases only lately recognized by medical science exact a heavy toll from the vitality of the people.

In the highly charged emotional climate of the Test Act controversy, thinking about the race issue was liable to distortion for another reason, which Disraeli's memorable phrase recalls. Briefly, the uprisen ape was by no means a fallen angel. Provoked by the irrationalities of their opponents, biologists thus found themselves sorely tempted to justify the inclusion of Man in the animal kingdom by recourse to arguments more menacing to the credentials of their antagonists than relevant to the rationale of a taxonomical preference. Inevitably in the heat of debate, it seemed all-important to emphasize what Man shares with other animals, the more so because so much about Man's peculiarities still eludes what we ordinarily agree to call physiological inquiry.

To be sure, the new evangelists had good reason to believe that differences with respect to the genetic make-up of animal species are primarily responsible for differences of behaviour which distinguish one (e.g. a social ant) from another (e.g. a solitary bee). Thus the analogy between locally restricted species of social organisms with distinctive anatomical *facies* and human communities distinguishable both by different culture patterns and by minor somatic peculiarities such as of skin colour or hair, disposed of any embarrassing temptation to remedy lack of intensive study of what is peculiar to the human ecological system.

Nor need we blacken the good name of our illustrious forefathers for what should now seem to be rashness of judgment on such matters. Our situation is otherwise. No one with educational pretentions still subscribes to Lightfoot's chronology, which dated the appearance of human life on this planet as September 12, 3298 B.C., at 9 a.m. With Lightfoot, Ussher and Bishop Wilberforce so far in the rear, and with no less detachment than that with which our predecessors could examine what distinguishes the mussel or the marmot each as one animal species from all others, we ourselves ought now to be ready to ask: what distinguishes Man from other animal species? Admittedly, we have still far to go before we can comprehensively discuss in the language of physiology the characteristics which make Man unique; but we shall not fulfil our target requirements (if at all) unless we recognize what they are.

In broad terms, they are easy to state and commendably biological considerations do not discharge us from the obligation to do so. Man is uniquely educable. In a unique sense, Man is a tool-making organism. In a unique sense, Man is also an animal capable of *informative* communication through speech. Because of this threefold uniqueness, a single animal species can fashion a changing environment and hence a changing milieu for its own developmental process. Like any other organism, Man transmits his genes to the next generation; but for the three reasons stated, and in a sense which likewise transcends anything comparable that we may rightly say of any other species, Man also transmits experience to the next generation.

Thus every change of the human environment through human interference signalizes a new accretion of transmissible experience and a new potential of further change. Because of this, human society is a unique ecological system. It owes its essential peculiarities to idiosyncrasies on which the study of social Hymenoptera has little or no bearing. While there is admittedly a *prima facie* case for the assumption that other local differences of animal behaviour are finally traceable to differences within the proper province of genetics, there is no such case for the presumption that different patterns of Man's social behaviour are predominantly traceable to the same source.

In the last resort, the mutation of chromosomes or of single genes is admittedly the pace-maker of organic evolution. We now know what the circumstances which determine its tempo and character include. These are:

(a) the rate of mutation;
(b) the viability of mutant types *vis à vis* the immediately available environment or the secular changes of the latter;
(c) mating systems more or less propitious to the concentration of genotypes in pure lines in a particular ecological *niche*.

We also know, though only since 1920, that mutation rates are highly variable. Consequently, the circumstance that many species have remained in all detectable ways fixed throughout vast geological epochs confronts our view of the evolutionary panorama with no enigma. Darwin's contemporaries and immediate successors preferred to ignore it. More especially as interpreted by Romanes and Galton after Weismann had discredited the Lamarckian supersti-

tion, the exponents of Natural Selection presented it as a process of ubiquitous, continuous and uniform change. Accordingly, the possibility of social change without concomitant organic change of comparable magnitude seemed to be contrary to the Laws of Nature. On the other hand, the possibility that *Homo sapiens* is a relatively stagnant species from the genetic viewpoint is no-wise repugnant to what we now know as surely as we also know that the human ecological system has a *momentum sui generis* regardless of concomitant selection of genotypes. No reasonable and informed person doubts that human beings are genetically variable; but a rational examination of the relation of transmissible patterns of human behaviour both to the diversity of the external environment in time and space and to the systems of mating peculiar to local communities must take within its scope a multi-dimensional potential of change attributable to the circumstance that one generation passes on to the next its own experience and the experience of its predecessors.

A single example should make this manifest. If the beginnings of civilization testify to the formative role of the calendar in the first stages of writing, they also disclose, and with equal eloquence, how latitude, climate and contour have been peculiarly propitious or otherwise to the universal necessity of time-keeping in communities which have refined the technique or have failed to do so. Though we still know very little about the genetic endowments of human communities and most that we hear is suppositious, such manifest external circumstances favourable to cultural efflorescence and to cultural stagnation are manifold. Nor can we appreciate how vast a range of possibilities they endorse if we discuss them singly and in isolation from the sum of acquired experience on which a particular community can draw. When the migrations of human stocks bring them into contact with otherwise similar circumstances, it will rarely if ever happen that two communities will respond within a comparable framework of traditional behaviour and equipment. Whether new circumstances are favourable to human inventiveness or otherwise, and if favourable with what possible outcome, is an enigma which therefore subsumes vastly diverse admissible solutions.

Thus the interplay of the diversities of environment on the stock-in-trade of transmissible experience encompasses a wide range of possibilities *vis à vis* the tempo and character of social change. Indeed, the inertia of experience accumulated in dealing with a

stimulus-complex which Toynbee calls the challenge of the environment may more or less effectively resist the impulse to deal with a new stimulus in a new way when other means of doing so are available. Accordingly, one may cite numberless examples of how failure to take advantage of a new situation may then deprive a community of the means of meeting the challenge of a different and later situation. For instance, the consequence of access to abundant root crops or of migration into an area where domesticable ungulates are available as beasts of burden may be quite different, if the event follows, from what it will be if the same event antedates, a well-established cereal economy. Equally, the adoption of a maize rather than a millet economy may initiate a train of events along a course with peculiarities—some medical—of its own.

A little reflection on widely accessible and abundant sources of information should therefore suffice to justify the conclusion that the joint relation of the human personality to its social and physical environment admits of many degrees of freedom, that minor variations of the sequence of otherwise similar stimuli may lead to widely divergent responses, and that anticipation of future consequences from definitive antecedents is rarely (if ever) a profitable undertaking. That the human ecological system has unique features, that it has a well-nigh limitless potential of change in the absence of the operation of forces which make some animal and some plant species more short-lived than others, and that genetic variability is never manifestly the pace-maker of such change are indeed propositions attested by the proper study of mankind. No knowledge we have yet gained from the study of plant or animal breeding can nullify them. Accordingly, such knowledge is not necessarily relevant to the evaluation of the changing character of human society. It would be rash to deny the possibility that genetic selection has played a part in the decline of civilizations, as asserted by R. A. Fisher and the late unlamented Alfred Rosenburg; but it is reckless to assert that it has done so without a searching examination of other possibilities, the more so when an assertion, itself perhaps plausibly relevant to a single instance, embraces the history of all civilizations.

Are we then to conclude that biology can make no contribution to the elucidation of circumstances contributory to the diversity of culture patterns more or less highly characteristic of human beings with predominant physical attributes in a particular locality in a

particular epoch? Assuredly not, unless we concede the impertinent claim that genetics embraces the entire field of biological inquiry or subscribe to a still too widely current nineteenth-century attitude to technological progress in antiquity. In the first fine flush of an unprecedented sequence of inventions following the introduction of steam power and the elucidation of the electric current, it was easy for archaeologists to take an unduly teleological view of the origins of fire, implements, clothing, the calendar, writing and especially (as the term implies) the domestication of animals. If we dispense with the inclination to do so, we are free to regard advances of human technology during the greater part of the 25,000-year saga of our species as a succession of fortuitous blunders dictated by unforeseen circumstances. In so far as this may be true, the *Führerprinzip* drops out of the story as an irrelevant postulate.

We have then to re-examine from two points of view the meaning of what we have hitherto called domestication *vis à vis* the role of commensalism in the diversification of what I have here called the human ecological system, meaning thereby what biologists customarily mean in contradistinction to the parochial and restricted use of the term in the title of the Professor of Human Ecology in Cambridge University. The beginnings of this composite system of species relationships takes us back to the association of Man with interfertile local varieties of Canidae in the early Palaeolithic, and hence to the possibility of blundering into herdsmanship where: (a) hunting nomads accompanied by their dogs came into contact with gregarious ungulates; (b) there were natural barriers to circumscribe an enclosure in which to round up a herd. From one viewpoint, we may therefore ask how the inclusion of one species in the ecological niche we call a local culture determines the subsequent inclusion of another. For instance, we may plausibly examine the sequence: given grain storage, then mice or rats, then the cat.

From a different viewpoint, we may ask: what new mechanical problems does the inclusion of a new species in the human ecological system force on the attention of the social group? For instance: is it a mere coincidence that the indigenous Americans without horses when Europeans came to their continent had not perfected the wheel? In *Habitat and Economy*, Daryll Forde has commendably put before us many problems of this type; but there is still considerable scope for fruitful co-operation between archaeologists, cultural

anthropologists, zoologists and botanists in the search for answers to many others. In my view, we shall advance little towards a deeper understanding of the diversification of human culture patterns in space and time till we are able to answer them. To be sure, human ecology in the very restricted sense of the term, as used by the Medical Faculty of Cambridge, had its own role to play until we know far more than we know as yet about what obstacles to technological advance are attributable to harmful organisms as members of the human ecological system in the wider sense of the preceding discussion.

On the other hand, the time is long overdue to recognize that the Pearsonian discipline designated physical anthropology, conceived in any terms other than its relevance to stock-taking in the tailoring and furnishing trades, is a blind alley in the landscape of biological science, like its parent phrenology harmless as a hobby for the opulent aged, but with no rational claim to support from the public purse.

2

THE ORIGINS OF SPECIES[1]

IT has long seemed to me a misfortune that Darwin called his best-known work *The Origin of Species*. The word 'species' has no single meaning. Hence there is no one problem of the origin of species. There are many problems of the origins of species. Having said this, I shall not suggest that there are only two. At the same time, it is useful to distinguish two broad categories which belong to different social contexts in which the problems of species origin have been discussed. A group of living creatures with a unique combination of common characteristics may preserve its identity and continuity as such for reasons which may be *extrinsic* or may be *intrinsic*. Since it is not the business of the taxonomist to probe very deeply into what these reasons are, there is no justification for supposing that the origin of the types of breeding unit which the taxonomist distinguishes as a species constitutes a single class of problems. In different contemporary or historic situations extrinsic may be more relevant than intrinsic circumstances to the taxonomist's task, and vice versa.

The naturalists of the eighteenth century, still largely immersed in the historic task of the herbalists, were predominantly preoccupied with differences between creatures living together within a comparatively restricted locality. Partly for this reason and partly because biology was still emerging from superstitious beliefs about metamorphoses, they were specially concerned to emphasize the fixity of species. In so far as they permitted themselves to speculate about origins, their problem was how systematic units which are distinct as *breeding units* came to be so. In the period of rapid colonial expansion which followed Cook's voyages, the Napoleonic wars, and the subsequent introduction of steam navigation, another attitude, that of the explorer-naturalist, prevailed. Systematic units which the

[1] Revised and expanded from a symposium *The New Systematics* (1940), edit. J. S. Huxley, Clarendon Press, Oxford.

traveller-naturalist meets in different countries are *ipso facto* breeding units. What interests him is not how species come to be *distinct*, but how they come to be *distinguishable*.

Himself a traveller-naturalist, Darwin approached the problems of species origin with this orientation. Hence his main preoccupation is with the action of *external* agencies which favour the survival of individuals with particular hereditable characteristics. Restated in modern terms, his theory is as follows. If a species inhabits a given area, the spread of a mutant character will be determined by the distribution of external agencies propitious to its persistence or otherwise. At different polar extremes of the same region, climatic and vegetative circumstances will conspire to build up different combinations of mutant characters. In course of time, the population thus becomes topographically dissociated. Darwin believed that if this goes on long enough species isolated by the physical segmentation of the environment might become intersterile. There was no direct evidence for this when Darwin wrote the *Origin*, and many of his opponents seized on the absence of evidence as a fatal objection to his theory. We have no difficulty in accepting it today. Intersterile stocks have arisen under experimental observation, and such work as that of Reed and Gordon on the Mexican Killifish shows us how a gene-substitution whose effect is merely ornamental in one gene-complex can be lethal when introduced to another.

In his own time the supporters of Darwin's doctrine paid little attention to criticism of the sort last mentioned. They were looking at the problems of species from a different angle. The fact that species may be biologically distinct units in experimental conditions as well as topographically distinct units in nature, is not the essence of the species problem from the standpoint of the traveller-naturalist. He encounters species in conditions which guarantee their separate existence, and the data of his problem are the differences by which they are distinguishable as such. The taxonomist, who follows in his footsteps, has not the equipment to test whether dead specimens sent to museums from different localities belong to intersterile stocks. So the concept of species as a breeding unit in this sense has scant relevance to museum practice. The distinction between geographical varieties and species defined in terms of intersterility is often arbitrary, especially when museum practice does not have to meet the requirements of horticultural importation. Thus a large number

of species included in at least two genera of the Canidae produce fertile hybrids. In captivity, interspecific and even intergeneric crosses of ducks, of pheasants, and of moths have also been found to produce fertile hybrids which illustrate the principle of segregation.

We can sometimes see this process of extrinsically determined species differentiation in its incipient stages. Thus each of the three interfertile races of the polymorphic species *Quercus robur* L. (*pedunculata*, *sessiliflora* and *lanuginosa*) has distinct local foci, although there is much overlapping. Sometimes also there is direct evidence for the survival value of characteristics which distinguish local races which are not sufficiently different to be placed in distinct species. Thus Cuénot cites experiments in which species of *Echium* from the Canaries grown side by side with a local French strain were killed by the first frost, which did not hurt the latter.

The physical segmentation of a population polarized in this way does not necessarily imply geographical isolation. A population may spread into different habitats so that different selective agencies operate according to the choice of host, of food, of dry, humid, cold, exposed, or shady conditions, and the prevalence of other animal species as prey. Cryptofauna, parasites, gall-forming and phytophagous animals illustrate this type of species differentiation, as do many related aquatic species which flourish at different levels of salinity, e.g. *Cardium edule* Linn., *C. exiguum* Gmelin, and *C. ovale* Sowerby, or in stagnant, running, upland and lowland waters, e.g. *Cambarus virilis* Hag., *C. immunis* Hag., *C. diogenes* Girard, and *C. monongalensis* Ortman. Polymorphic species of plants furnish examples of all stages in the process of separation. On the borderline of complete separation we find forms such as *Dianthus deltoides* L. with solitary flowers, and *D. armeria* L. with a dense inflorescence. These two species are respectively confined to sandy uplands and loamy soil, so that their fertile hybrids are rarely found. Thus the traveller-naturalist is fully entitled to extend his hypothesis over a much wider field of species differences, guaranteed in the formative stage of species differentiation by extrinsic separation. If all species which are clearly distinct entities were confined to a restricted habitat or locality, and if closely related species were never found living side by side, or were found to do so only in circumstances pointing

to a previous separation, we should be entitled to accept the mathematical theory of natural selection as an all-inclusive hypothesis. We might then dismiss lightly Bateson's plea for the experimental study of species intersterility.

Needless to say, we might also be entitled to make reservations. Some of these have been frequently stated by naturalists. For instance, the alleged selective value of differences between closely allied species, e.g. the blue and the brown hare, sometimes demands an effort of imagination, if not an act of faith. Against the background of the immense variety of extrinsic agencies which favour the survival of the fittest (i.e. the survival of those that survive), the notion of competition for the means of survival is merely a political rationalization. For instance, the topographical accident of host choice may be of immense evolutionary, as assuredly of practical significance in connection with the deliberate introduction of alien animal species for food or fur into a new environment. Why does the native British squirrel disappear so quickly when the imported American grey squirrel invades its territory? That shortage of nuts for the nutriment of both *en rapport* with the *laisser faire* rationale of nature red in tooth and claw is the least credible of conceivable answers to the question. Far more likely, one explanation is on all fours with what we know about the susceptibilities of turkeys and domestic fowl to Gapes. The last named is a thread worm (*Syngamus*) no more harmful to turkeys than is pin worm (*Oxyuris*) to boys and to girls. It is fatal to fowls, and this story (unlike that of Frankie and Johnnie) has a moral. On the threshold of independence, Ghana was importing by plane day-old chicks of *Gallus domesticus* all due to die of tropical parasites before each had laid more than two dozen eggs. The tropical and resistant Guinea fowl has already an egg-laying level on all fours with the records of our domestic fowl on the eve of World War I.

For many reasons, this enlargement of the Darwinian thesis cannot be the whole truth about the historical panorama of evolution. *Inter alia*, we are entitled to wonder whether it is not a little previous to assign an upper limit to mutation-rates unless we are assured that laboratory conditions reproduce all the conditions propitious to the transformation of genes in formative stages of species differentiation in nature. Up to a point, mathematical analysis of evolution is therefore a healthy discipline, because it can at least

tell us whether selection could *not* have produced observed structural differences when the conditions postulated by the hypothesis are fulfilled and Professor Haldane's suggestive treatment of phenomena supposed to support orthogenesis illustrates the use of such calculations.

At the same time, we should not forget the highly arbitrary assumptions of any formal mathematical treatment of selection. Without doubt natural selection is a very important feature of species differentiation, so long as our concern is with the class of problems discussed so far. What I am here concerned to emphasize is that another class exists, and if its existence has often been overlooked by experimental geneticists it has never been denied by field-botanists. In contradistinction to the traveller-naturalist, the field-naturalist who stays at home is impressed by the fact that closely related species are often found side by side in nature. Circumstances sometimes suggest that they may have been selected for separate topographical situations or separate ecological units which have since coalesced. Often there is much to suggest that no such separation has occurred. When this is so, the fact that they do not interbreed is no longer a subsidiary issue.

Bateson's special contribution to post-Darwinian discussion of selection was the recognition of this class of problems. Though he sometimes chose his words *pour épater le bourgeois*, there is little doubt about his main reason for criticizing the mechanical view that all evolution is the interplay of mutation and mortality, or for emphasizing the need to study the intersterility of closely allied forms. As a field-naturalist, he was specially interested in the co-existence of closely allied species in the same habitats. *The Problems of Genetics* harps on this theme. To anyone familiar with plant life the spectacle of several different species of Potentilla, Geranium, Veronica, Ribes, and many other genera growing side by side is a commonplace. Cuénot points out that Darwin's theory met with more contemporary criticism from botanists than from zoologists, among whom the standpoint of the traveller-naturalist predominated at the time.

A group of animals I once studied from the cytological standpoint provides a clear-cut illustration of the evolutionary problem from an alternative standpoint. Cameron recognizes about sixty-seven species of gall-forming Cynipidae. Of these fifty-four, or

eighty-one per cent of the total, infest a single plant species, the *oak*. As for the remaining thirteen named species, twenty per cent of them infest the rose. Those who are content with the simple mechanical view which reduces all evolution to the interplay of mutation and mortality are entitled to point out that all oak cynipids do not infect the same vegetative structures. While this is true, it is equally true that many do so, and, as far as I know, there is no noticeable preference of closely related species for a particular type of oak tree. It is common to find several kinds of galls on the same leaf. *Neuroterus lenticularis* (Olivier), *N. numismatis* (Four.), and the cherry-gall *Dryophanta scutellaris* (Olivier) are often found together on the same tree and on the same leaf of the same tree.

Analogous remarks apply to the inquiline species or guest-wasps of the same family. I do not believe that any naturalist familiar with the Cynipidae would seriously entertain the possibility that this great proliferation of species in one and the same ecological situation is compatible with selection for different habitats which have subsequently coalesced. On the other hand, the facts are not so difficult to explain if we take into account a species characteristic which does not and cannot yet enter into any formal mathematical treatment of evolution as a whole. All the gall-forming Cynipidae have an agamic generation, usually alternating with a spring or bisexual brood. The latter is sometimes eliminated so that the species is entirely parthenogenetic. This is true of the marble-gall *Cynips kollari* Hartig, of the moss-gall *Rhodites rosae* (Linn.), of at least four species of *Andricus*, and probably one of *Dryophanta*. The absence of sexual forms in five or six other species is more doubtful.

The possibilities of further variation in the descendants of an individual member of a species depend on two things, its own gene-complex and opportunities for interbreeding between its own progeny and individuals with ancestors endowed with a gene-complex different from its own. In so far as these opportunities are not restricted, selection will ensure that a particular gene-complex eventually becomes distinctive of a particular population. If the opportunities for interbreeding are restricted the spread of mutant genes is also restricted. Selection acting on different stocks may thus produce a variety of different types in the same environment. Species-differentiation then proceeds as with widely different species in the same habitat, and *the condition of this does not lie in*

external nature. It resides in the biological make-up of the type.

In contradistinction to the extrinsic aspect with which the theory of natural selection is concerned, the intrinsic aspect of evolution is concerned with characteristics of the biological make-up and habit of animals, in so far as they promote the possibility of species-differentiation in one and the same habitat. In a limited sense Darwin anticipated the need for a distinction of this kind by contrasting *natural* selection with *sexual* selection. Owing to gratuitous assumptions on which theories of sexual selection have been erected the latter has now been pushed into the background. The result is that mathematical geneticists either claim for selection far more than Darwin did himself, or use the term in a totally different sense.

A certain class of mutant characteristics, of which the power of agamic reproduction is only one example, introduce new evolutionary potentialities which justify a new name. In the Hegelian terminology, which I rarely use, they illustrate the passing of quantity into quality. The mere fact of giving them a separate name directs attention to the need for researches which are otherwise liable to be neglected. Difference of opinion in this context is largely a matter of perspective. No one denies that one species (e.g. the black rat) may invade the territory of another (e.g. the brown rat). Likewise no one denies that speciation in one and the same environment does *sometimes* occur. What some zoologists—in contradistinction to most botanists—would maintain is that the evolution of different species in the same region is comparatively rare, so rare that the broad outlines of the evolutionary drama emerge with sufficient clarity, if we leave it out of account.

Here it seems to me is a vital difference between evolution as the traveller-naturalist or the zoologist trained in type morphology sees it and the evolutionary problems which concern the field-ecologist. If you are content to lump a stretch of moorland with its ericaceous flora, a chalk down with its varied assortment of blue butterflies, a coral reef, an oak infested with gall-flies, or a bat cave, as exceptional phenomena, or if you are prepared to argue that all the related species coexisting in these situations have been differentiated by selection for slight differences of habitat, it is permissible to hold that the origin of species is mainly determined by natural selection in Darwin's sense. If not, you have to include, among the formative agencies of evolution, intrinsic biological circumstances which pre-

determine the subsequent operation of external selection for a particular combination of genes.

For instance, various chromosome mutations (polyploid, polysomic, and inversions) are associated with partial or total intersterility with respect to parent stock. In the more extreme case a mutation of this kind can survive only if the individual is capable of perpetuating itself. This it can do if it can reproduce by clones, as do so many denizens of a coral reef, if it is a hermaphrodite capable of self-fertilization, as are so many related polyploid species of flowering plants, or if it is parthenogenetic, as are some elementary species of *Hieracium* or some animals like aphids. If these or other (*vide infra*) conditions are not realized at the outset, selection is powerless to ensure the survival of the new type. In brief, selection is not the explanation of the formation of new species in this way. Although species capable of vegetative reproduction, parthenogenesis, and self-fertilizing hermaphroditism are rare among animals, they are not collectively negligible, and I shall draw attention to other, possibly more common, biological mechanisms which act in the same way.

Long ago, the frequent occurrence of very slight differentiation of forms, which preserve their identity as breeding-units while living side by side, compelled botanists to draw a somewhat arbitrary distinction between elementary species or *jordanons* and major species or *linneons*. In many cases the biological prerequisite of species-dichotomy without topographical or ecological isolation is fully understood. Thus the crucifer *Draba* (*Erophila*) *verna* L., with over 200 elementary species, of which it is possible to find as many as a dozen growing within an area of a few square yards, includes forms which are apogamous and others which are hermaphrodite and cleistogamous. Artificial crossing of the latter may give fertile hybrids which produce apogamous types from which new pure lines can be cultured. Others give sterile hybrids. Either way their separate identity as distinct breeding-units is guaranteed by the fact that self-fertilization is obligatory or fertilization is unnecessary. If obligatory self-fertilization occurs, mutants which are intersterile with respect to parent stock are privileged. Their survival is not compromised by the unlikelihood of finding a suitable partner. Cuénot says that the three overlapping and very closely allied species *Viola arvensis* Murr., *V. nana* Godr., and V. *kitaibeliana* Roem. & Schult., include at least 5 intersterile races with 14, 16,

24, 36, and 48 chromosomes. Polysomy and polyploidy in this group is again associated with cleistogamy.

There is no need to catalogue the immense number of allied plant species which are of polyploid origin. Their existence is instructive for two reasons. The first is that chromosome- (as distinguished from gene-) mutation is often associated with very striking structural differentiation and with a high degree of intersterility with reference to parent stock; and chromosome-mutations thus furnish *an indisputable example of species-formation without the instrumentality of selection.* The second is that their frequent occurrence is associated with optional or obligatory self-fertilization or with apogamy. Hermaphroditism is the rule in flowering plants. Contrariwise, polyploidy, which is so common in Angiosperms, is very rare in Gymnosperms.

Obligatory self-fertilization is extremely rare in animals, and optional self-fertilization is not the rule among the small proportion of animals which are hermaphrodite. This may be why polyploid species are extremely rare in animals. The few indisputable examples occur in agamic species of Isopods and Branchiopods. Vandel has shown that there are two overlapping geographical races of *Trichoniscus provisorius* Racovitza, of which one is bisexual and has sixteen chromosomes. In the other, which has twenty-four chromosomes, males are very rare. The parthenogenetic race of larger build is a typical triploid of the type common in flowering plants. In *Artemia salina* (Linn.) Gross has distinguished between diploid, tetraploid, and octoploid races. The diploid races may be bisexual or parthenogenetic. Tetraploid or octoploid races are only parthenogenetic.

The fact that parthenogenesis, like obligatory self-fertilization, is rare among animals does not mean that intrinsic species-dichotomy without differentiation is exceptional in animals. A mutant intersterile with respect to parent stock may produce a new race if it can bud off individuals of either sex. Such propagation by clones is very common among sedentary animals, of which the hydroid fauna of rock pools and the coral population of a reef provide examples of closely related species in the same habitat.

Superficially analogous to clone production is the phenomenon of polyembryony. This is extremely rare. The best-known examples occur in Gymnosperms, parasitic Hymenoptera, Cecidomyidae, and

armadillos. Except among the first named among which many species are monoecious, this does not ensure that mutant recessives intersterile with parent stock can propagate. However, it does ensure the multiplication of heterozygotes with a mutant gene, and may therefore conceivably favour the emergence of such recessive types.

Two other mechanisms of intrinsic differentiation are less sharply distinguished from the interplay of natural selection. One is delimitation of sexual periodicity. The other is sexual discrimination. When fertilization is external and vegetative reproduction does not occur, the first is the only method of establishing new species in the same habitat. If sexual congress takes place, mating may be restricted to particular times of the year. Intrinsic species-differentiation by variation of the mating season is easily confused with selection as usually defined. This is because minor species distinguished by their mating or flowering-seasons are often restricted in their habitats. For instance, the cowslip and primrose, placed by Linnaeus in the same species because their fertile hybrids are sometimes found where their ecological domains overlap, commonly behave as distinct assemblages kept apart by two circumstances. One is the fact that primroses prefer the shade of the hedgerow or wood, while cowslips grow in open meadows. The other is that the flowering-season of the cowslip does not begin till that of the primrose is almost over. The shift in the time of flowering is almost sufficient to make the two forms clear-cut species, and their ecological distribution is almost discontinuous. Hence it is impossible to say whether the former is primary and the latter is secondary, or vice versa. The same remark may apply to a pair of species which are fully separated e.g. the two moths *Eupithecia innotata* Hufn., which feeds on *Artemisia*, and *E. unedonata* Mab., which emerges much earlier, on *Arbutus*. By cooling the pupae of the *Arbutus* species, Dietze delayed emergence and produced fertile hybrids with the *Artemisia* species.

The seasonal character of the reproductive process in other distinct or incipient species which are closely related is clearly antecedent to ecological or topographical disintegration of the external environment. Two slightly differentiated races of the 17-year Cicada (*Tibicen septemdecem* Linn.) probably belong here. Apart from the fact that the single brood of *Lysandra coridon* Poda emerges between the two broods of *Lysandra bellargos* Rott., there is no known obstacle to interbreeding between these two butterflies. When, as

in many Lepidoptera, a sharp seasonal difference of mating accompanies a difference of food plant, the ecological isolation of the larval stage offers no obstacle to interbreeding, unless the plants themselves belong to different associations.

The need for ecological research into species-formation from this point of view is illustrated by a survey of Lo Bianco, who has tabulated pairs of species belonging to 37 genera of molluscs and 52 species of crustacea from the same area to ascertain how far their breeding-seasons overlap. In both classes he found that 21 ± 2 per cent of the pairs examined had quite distinct times of spawning. In 63 per cent the periods overlapped. In 15 ± 2 per cent they were co-extensive. It would be instructive to take a limited region of this kind, compare the number of related species with and without association of the sexes, combined with or without clone production or parthenogenesis, and tabulate side by side with these figures the coincidence of seasonal reproductive activity.

Another type of intrinsic species-differentiation is akin to Darwin's unverified sexual selection, and any other form of assortative (i.e. non-random) mating. The only analogous phenomenon in plants is the preference of insect species for flowers of one or another kind, e.g. the preference of bees for shallow blue flowers and of night-flying moths for white flowers with a tubular corolla. Sexual discrimination is conditioned by the biological make-up of animal species in a variety of ways. It can occur only in species of which individuals of opposite sex associate in an act of coitus. This excludes all species which are agamic, all species which are sedentary like coelenterates or polyzoa, and a large proportion of mobile marine invertebrates such as echinoderms and polychaetes. It also presupposes a certain level of evolution in the receptive mechanism of behaviour. In gregarious species or species which live in small family groups it has greater potentialities, especially if sexual behaviour is conditioned by previous experience. What is important in this context about sexual discrimination, i.e. any kind of assortative mating, is that it increases enormously the chance that a new variation will be established as a pure line. Sexual discrimination may depend on structural differentiae common to both sexes. It has been shown that a definite preference of like for like exists when white-eyed mutants and wild-type individuals of *Drosophila* are bred together.

Differential virility in sexual congress is another type of assortative (i.e. non-random) mating illustrated by the partial barrier between the white mouse and the house mouse. The most obvious form of sexual discrimination involves selection for sex-limited characters. Of all groups in the animal kingdom, butterflies and birds provide the most prevalent illustrations of sexual dimorphism and therefore presumably of assortative mating for sex-limited characters. May it not be significant that these are the two pre-eminently successful groups equipped for aerial locomotion? The reproductive environment of an aerial—even less perhaps than that of an aquatic—organism is least susceptible to ecological or topographical isolation. So they should be least subject to natural selection, by which I mean what Darwin meant. The interplay of intrinsic and extrinsic aspects of evolution is illustrated by the possibility that a mutation affecting beak structure may favour the choice of a different diet, hence of a different situation as a meeting-place for similar individuals. In spite of this, the great aerial groups contain an exceptionally large number of related species, and the association of closely related species, such as the blues or heaths among the Lepidoptera, in the same habitat is quite common, indeed specially striking, among them.

Contrariwise the bats, which do not exhibit extreme dimorphism of the sexes, provide a plausible illustration of species-differentiation without topographical or ecological isolation, and might therefore be cited to refute the suggestion implied in the preceding remarks. It is possible and not unlikely that another type of sexual discrimination may exist in the bat cave. The researches of Zuckerman indicate that mammals display a peculiar specialization of sexual discrimination associated with the development of the forebrain. For lack of a better term we may call it tuitional sexuality. Tuitional sexuality in this context signifies the contribution of love-play between litter mates to the normal consummation of sexual habit. Its importance lies in favouring incest. Incest is a type of assortative mating which does not necessarily imply great dimorphism in the visible differentiae of the sexes; and it holds hitherto unexplored possibilities of promoting species-formation. For that reason our ignorance of the extent to which it prevails is regrettable. Fortunately it is also remediable. Analysis of wild populations for the frequency of homozygous dominants offers a straightforward, if laborious, method of ascertaining its prevalence.

The study of human inheritance draws attention to several relevant characteristics of close inbreeding. In general a rare recessive mutation (e.g. human albinism) occurs with relatively high frequency among the issue of consanguineous unions and it must also be *familial*, i.e. it occurs among several members of a fraternity whose parents do not exhibit the mutant character. When a single gene-substitution is involved the proportion of all recessives in a fraternity containing at least one recessive approaches a lower limit of 0·25. For litters of eight the appropriate figure is 0·277 and for litters of two it is 0·576. The study of rare recessive traits in human communities thus illustrates two principles. The first is that circumstances which encourage close inbreeding *in excess of what would occur in a system of random mating*[1] increase the frequency of recessive types. The second is that circumstances in which recessive types most commonly emerge are such that there is a *high probability of mating* inter se, *when little or no exogamy occurs, and the size of the family is small.*

The relevance of these considerations to one aspect of the problem of species-formation is not far to seek. It is easy to imagine that a recessive type (aa) may be interfertile with its own kind and intersterile with parent stock (Aa or AA). Such intersterility may be relative or absolute, structural, biochemical, or behaviouristic. Statistics of human nuptiality would probably reveal many categories of relative intersterility based on 'preference', and animal genetics provides factual examples of size differences which effectively constitute an absolute barrier to interbreeding in normal circumstances. In a system of random mating a rare mutation which is intersterile with parent stock, like any other rare mutation, has a negligibly small chance of mating with its kind and starting a pure line. When endogamy is obligatory this is not true. A rare and relatively viable recessive has a relatively *high* chance of mating with another of its kind and therefore of starting a pure line which will be a breeding-unit from its inception. In so far as recessive mutations may be relatively or absolutely intersterile with parent stock, endogamy therefore constitutes a new potential of species-formation in animals which produce few offspring at a time. In so far as evolution involves different mechanisms of *species-dichotomy* or

[1] The frequency of inbreeding increases at low density in a system of random mating without affecting the frequency of recessives (cf. Dahlberg *Proc. Roy. Soc. Edin.*, 1938).

intrinsic species-differentiation (as opposed to species-differentiation dependent on extrinsic causes), how species originate involves different problems in plants and animals and different problems at different evolutionary levels in either of them. For instance, mosses and ferns have a higher potential of species-dichotomy than cross-pollinated flowering plants. In these phyla a single individual which is sexually incompatible with the parent stock produces a multiplicity of new sexual forms by spores. Hence it should not surprise us to find many fern or moss species growing together in the same habitat.

A mutant which will not produce offspring when mated back to parent stock, or if it does so, will produce only sterile hybrids, can establish itself if one of several conditions holds good. From a genetical viewpoint, I shall formulate these more explicitly, when summing up this enquiry. So far, my concern has been less to emphasize exclusively intrinsic than to indicate the interplay of intrinsic and extrinsic circumstances on the separation of genotypically different units and of their preservation. As we have seen, restriction of the reproductive season and intense assortative mating in one form or another may also establish a partial species-barrier and enable species-differentiation to proceed by selection in the same environment from a different genic basis. Such a barrier may gradually become a complete obstacle to interbreeding in the environment, and we encounter all stages of this process in moths.

If evolution progresses with ecological separation, different inversions of chromosome segments, fusions, non-disjunctions, or fragmentations may occur in contiguous stocks, of which hybrids are less viable than their parents in the habitats where the latter thrive best. If this continues for some time, chromosome-mutations which do not militate against survival in competition with one parent stock may well do so with the other. Selection might then favour the separation of species into incompatible breeding-units. Thus selection would become the agent of new biological barriers to the interbreeding of species. This could be true only at a low level of ecological isolation permitting frequent hybridization to occur. On the other hand, species-differentiation associated with geographical isolation does not imply the slightest selective advantage for biological obstacles to interbreeding. This may be why very striking species-differences between localized species may coexist with complete interfertility.

One corollary of this is the futility of controversy about whether Man or the dog is polyphyletic in origin. Several local 'species' of the dog family, like 'races' of mankind, are interfertile products of geographical specialization. In so far as extrinsic barriers separate them into distinct assemblages, they may gain taxonomic rank as species, and they remain so as long as the extrinsic barriers are there. So the taxonomic distinction is relevant to a certain level of transport industry, for instance the stage at which human beings could convey with them their commensal Canidae in canoes. In so far as this is so, the origin of dog species is less a problem of biology than a problem of economic history. The sterility of discussing the polyphyletic origin of man is analogous, and also illustrates how difficult it is to think clearly about matters which involve social preferences.

That the method of species-formation differs vastly at different levels of evolution is specially emphasized by a comparative study of the genetic architecture of animals and plants. One illustration has been given. Polyploid species are common among plants and very exceptional among animals. In some groups of animals the chromosome numbers vary little. Thus all the Urodeles recorded in Ethel Browne Harvey's monograph have twelve pairs of chromosomes with no striking sexual differentiation of an X-pair. Nearly all the species listed are geographically distinct. Apparently all primates have 23 or 24 pairs. Lepidoptera and birds are two groups in which the chromosomes are numerous and highly variable. No attempt so far as I know has been made to link up these variations of the genetic architecture of species with the circumstances of their occurrence in nature.

Perhaps the most striking feature revealed by the summaries which have been published by Ethel Browne Harvey is the occurrence of *formative* genera or species which show considerable variation in groups which are otherwise homogeneous. Out of 13 families of Diptera, excluding the family to which the genus *Drosophila* belongs, all the 20 recorded species of 9 families have 12 chromosomes. In all, 24 species or 65 per cent of the 37 listed, distributed among 21 genera and 13 families, have this number. One family, the Culicidae, includes 4 species, all of which have 6 chromosomes. This accounts for 28 species of Diptera. The remaining 9 species, having 8, 10, 14, 16, and 18 chromosomes, occur in families which accommodate other species having 12. Such uni-

formity is in striking contrast with variability within the limits of the single genus *Drosophila* which contains species with 6, 8, 10, or 12 chromosomes, the numbers 8 and 12 being modal. The Orthoptera listed by Ethel Browne Harvey are also instructive from this standpoint. Fifty-six out of 67 listed species of Acridiidae have 24 chromosomes. In the Locustidae the numbers 24, 30, 32, 34, and 36 occur with no noticeable modality. In the 6 families including, besides the two last-named, the Blattids, Gryllids, and earwigs, 62 out of 83 species have the number characteristic of the Acridiidae. Closely allied homogeneous assemblages of the same phyletic rank are thus found at every level of classification side by side with assemblages of species or even—as in Coreid bugs—of subspecies displaying great variability in the architecture of the germ-cells.

In current text-books such differences are sometimes stated to furnish an explanation for the existence of sterile hybrids. For instance, it is often said that the common mule is sterile because the chromosomes of the horse and donkey differ in number and size. The problem of how they came to be different is therefore shifted into the Miocene, where there is no temptation to undertake the inconvenient task of solving it. Similarly, a text-book explanation for the intersterility of *Drosophila melanogaster* Meig. and *D. simulans* Sturt. is the existence of a large inversion on the third chromosome. Presumably this is due to a succession of small inversions of the type which arise after X-radiation. Such inversions may be associated with partial intersterility which might become complete if several occurred successively. This may well have happened. The view that selection of *favourable* mutants furnishes an all-sufficient explanation of evolution does not help us to see why it should have happened.

We are thus left with a category of specific differences for which neither selection nor any of the mechanisms of intrinsic species-differentiation so far discussed offer a satisfactory explanation. In *Drosophila obscura* Fln., Lancefield has isolated two races which are intersterile in nature. They differ in the size of the Y-chromosome, overlap in their distribution, and are hardly distinguishable in other respects. How have they arisen? It seems to me that we can choose between two possibilities. If they are partially adapted to the habitat which they prefer in ways which we cannot distinguish, and if their hybrids were less adapted to either, at a stage when they were still

interfertile, it is possible that selection contributed to their present condition. On that view, the fact that the Y-chromosome of one race is twice as large as the Y-chromosome of the other is not the reason for their intersterility. If the true explanation lies in the known occurrence of chromosome-mutations, we require a subsidiary hypothesis.

X-radiation has made it possible for Stern to isolate intersterile races in *Drosophila melanogaster*. Presumably the possibility of doing so lies in the fact that this procedure enormously increases the mutation rate. Since *Drosophila* is not agamic nor necessarily incestuous, nor hermaphrodite, and is incapable of reproducing by clones, we should expect that chromosome-mutations which result in partial or complete intersterility with respect to parent stock would have no chance of surviving. Stern's work suggests an explanation for the occurrence of chromosome species of the type which Lancefield describes, if we grant that circumstances in nature from time to time result in a frequency of mutation with no other parallel in the experience of standardized laboratory cultures. If so, the circumstances responsible for mutation itself are more significant than the action of selection in producing species which differ in the architecture of the germ-cells. Bacon said that the subtlety of nature is many times greater than the subtleties of our reasoning powers. So I do not, and have never, suggested that this is the only possible explanation of the facts. I do insist that the mechanical view of evolution as the interplay between external selective agencies and mutation rate raised to the status of a universal physical constant in the mathematical formulation of the theory does not offer a better one.

Since my own views have been grossly misrepresented in print by Julian Huxley who has never outgrown the father image of Darwin and the grandfather image of his devoted henchman, I propose to conclude this somewhat discussive survey by a point by point analytical synopsis. To discuss in a meaningful way how, and in many ways, so-called species originate we must start with the recognition that meaningful definitions of species must needs be diversely in keeping with the diversity of criteria which prompt taxonomists to greater or less refinement in the task of making a card index of different sorts of animals and plants.

Broadly speaking, it is true to say that systematists put organisms

in distinct species when their progeny display distinctive features from generation to generation. The way in which such units maintain their distinguishable characteristics is usually irrelevant to the task which the systematist carries out, but the way in which they maintain their distinguishable characteristics is the core of the evolutionary problem. The ultimate units of a classificatory system may be distinct because they are biologically incompatible or merely because they are topographically inaccessible. World-wide search for ornamental and useful plants has encouraged botanists to dwell on the first criterion, and the ease with which it is possible to transport seeds or bulbs makes it relatively simple to apply. Limited opportunities for studying the interfertility of animal types which live in different localities, limited powers of dispersal which animals possess, and interest focused on the geographical distribution of animals by researches of the great traveller-naturalists of the nineteenth century have encouraged zoologists to dwell on the topographical aspect of evolution.

Even if we adopt an alternative criterion and define a species as a group of interfertile members which do not produce fertile hybrids with members of other groups designated as such, there is no singular basis for the fact that closely related forms remain separate in one and the same environment. Biological incompatibility may depend on differences of the genitalia, chemical or structural characteristics of gametes, periodicity of the sexual association, or even habits of everyday existence. Consequently there is no one problem of the origins of species. Since the categories of taxonomists are dictated by practical considerations which have no necessary relation to those of the geneticist who works in the laboratory, a modern statement of evolution must go to the field to find common ground for discussion better than to talk about the origin of species. In Dahlberg's terminology, it is then clear that there is a more profitable way of defining organic evolution. *Evolution is the differentiation of genotypically distinguishable isolates.* This makes it clear at the outset that there are many ways in which evolution can occur. Consequently there is no single mathematical theory of evolution.

What mathematical analysis can do to clarify discussion is to elucidate the diverse ways in which genotypically distinguishable isolates become separated with special reference to the speed at which such processes occur and the limits to which they tend. In the absence of mutation, differential migration, inbreeding or other type of

assortative mating, a population remains constant. A population can separate into genotypically distinct and distinguishable isolates in at least four ways:

(a) Topographically by migration and selection in the sense defined above (p. 24) that is, by differential fertility or viability of genotypes in different ecological or geographical situations.
(b) Chronologically by mutation pressure, which may change the genotypic structure of a population in the course of time, even if no selection occurs.
(c) Coextensively in one and the same environment in three different ways, namely (i) inbreeding, (ii) assortative mating, (iii) chromosome mutations.
(d) Fortuitously, as emphasized by Sewall Wright, in so far as a mutation may establish itself locally in one region rather than another regardless of selective advantage.

No general formula subsumes all these agencies contributory to genotypic differentiation of isolates. Their relative importance depends on spatial and temporal circumstances on the one hand and the biological make-up of the ancestral stock on the other. Because the interpretation of evolutionary change since Darwin's time has been focused chiefly on (a), mathematical theories of evolution have been mainly preoccupied with the same issue. We can get a fresh perspective if we take stock of circumstances, which contribute to the relative importance of processes which have received more attention as the result of more recent research. Of such, chromosome mutations claim first place, both because of their importance, which is realized more and more, and because they emphasize how the role of any agency of evolution depends on the biological make-up of the group. Against the background of data assembled by Darlington (1940), we may confidently say that chromosome mutation is responsible for at the very least thirty per cent speciation among flowering plants, a conclusion which is scarcely consistent with the views of those pundits who assert that modern research has vindicated Darwin's doctrine of natural selection.

Chromosome mutations may entail major phenotypic differentiae deemed worthy of specific rank, and are often characterized by relative sterility of the mutant with respect to the ancestral stock. When this is so, the mutant can establish itself if the type can re-

produce by any method, other than bisexual reproduction, that is, by (a) parthenogenesis; (b) hermaphroditism, with the possibility of self-fertilization; (c) vegetative reproduction; (d) reproduction by spores. Among plants the existence of at least two of these conditions is the rule rather than the exception. Of animals the reverse is true. Polyploid species are rare among animals with a high grade of neuromuscular equipment, and occur chiefly among parthenogenetic species. They are very common among flowering plants. Indeed polyploidy or polysomy constitutes an agency of species formation, scarcely less important than selection. Even self-sterility of allopolyploids does not necessarily exclude survival. The widespread existence of sexually sterile forms which propagate by clones, as do species of Bougainvillea, shows that this may well be a mechanism of speciation.

Close inbreeding is an extremely rapid way of differentiating isolates with respect to gene mutations. The most extreme form, self-fertilization, is now generally recognized (see Cuénot's *L'Espèce*) as a clue to the production of elementary species (so-called *jordanons*) among flowering plants. Since most animals with a high level of neuromuscular co-ordination are dioecious, the closest form of inbreeding which is common among them is brother-sister or parent-offspring incest. This can be relatively common, if either of two conditions holds good: (a) the density of the population is very low, so that individuals have access only to closely related mates, (b) sex attraction is preconditioned by 'love-play' with litter mates. The second is possible only at a high level of neuromuscular co-ordination; but the first merits further comment. If a recessive mutant which is relatively intersterile with parent stock turns up in a comparatively dense population, the likelihood that it will mate successfully and establish itself is negligibly small. Consequently, it will die out. If the population is very dilute, the situation is not comparable, especially if the size of the family is small. The population density of some mammalian species, such as the otter or pine marten in Britain, is so low that congress between litter mates must be a common occurrence, if not the rule. When the size of the litter of such a species is two, algebraic analysis shows that one out of every seven families in which any recessives occur is made up of two recessives. If incest is obligatory, these recessives must interbreed. So if brother-sister mating is obligatory, or if common because of

low population density, the odds against two recessives mating are not great, and recurrent mutation offers the possibility of starting a pure line. This may be one reason for the epidemic speciation of the mammalia in the early Eocene, when the density of the world's mammalian fauna must have been very low.

Assortative mating, other than inbreeding, includes Darwin's hypothetical sexual, in contradistinction to natural, selection. Assortative mating of the active type can occur in animals only if the sexes are externally differentiated in association with the occurrence of sexual congress, and thus implies a particular level of neurosensory development. Entomophily in flowering plants also offers possibilities of intensive assortative mating. It is not likely that assortative mating is of great importance in the evolution of organisms, since it does not bring about great changes of the distribution of genotypes in a population, unless it is very intense. This must be a comparatively rare phenomenon.

No issue emphasizes the contrast between a modern view of evolutionary change and that of Darwin's generation more sharply than the role we ascribe to mutation pressure. To Darwin's generation, heredity and variation were coextensive. Since it regarded hybridization as a blending of hereditary materials, any deviation from the mode in one direction would be diluted out of existence by concomitant variation at the opposite extreme. Neither spatial nor temporal differentiation could therefore exist without the pressure of selection to truncate the tail of the supposedly normal curve of variability. Since the rediscovery of Mendel's laws, our outlook has changed for three reasons: (a) there is no indissoluble blending of the germinal stock-in-trade of the organism; (b) the same mutant types may turn up again and again; (c) at its first appearance a mutant gene is commonly protected against adverse selection by the dominance of its ancestral allele.

Mutations which have no selective value may become locally established by sheer luck. They may be established coextensively in pure lines, if parthenogenesis, or self-fertilization, clone production, or spore formation occurs. This may well be an explanation of the great proliferation of *Cynipid* species in one and the same environment (e.g. the oak). What is of far more importance is that a mutant type may eventually supplant the ancestral genotype by recurrent appearance. We have no exact figures of the spontaneous

occurrence of a new mutation and its reverse mutation; but work especially that of H. J. Muller, on artificial mutation by X-rays, now enables us to assign for some species a precise figure specifying an equilibrium when about ninety per cent of the population is transformed to the mutant type by the accumulation of the mutant gene in successive generations. Indeed, if several mutant genes evoke the same somatic effect, the complete obliteration of the ancestral type by mutation pressure is possible, and speedily on the time-scale of the evolutionary process.

One result of the exaggerated claims for natural selection put forward by Darwin's followers was the difficulty of explaining certain facts about non-adaptive relations between animals and their habitats, for example, the very large proportion of cave animals which have degenerate eyes. If it were conceivably possible to find any rational basis for such a semantic monstrosity as the so-called inheritance of acquired characters, an explanation of such phenomena would be facile, and the difficulty of deriving any satisfactory rationale for them from the standpoint of selection may partly account for the tenacity of the Lamarckian superstition. As far as we know, to have no eyes is of no advantage for an animal that lives in the dark, and though it is easy to see why some animals which have no eyes or have defective ones could manage to survive in caves, natural selection provides no explanation of why so many cave animals should enjoy this distinction. What we now know about *mutation pressure* offers a new approach to this problem. We know that both eyeless and defective-eye mutants occur in *Drosophila* and in other animals. We know that the same mutants turn up again and again. If there is no disadvantage in possessing defective eyes, the proportion of individuals which have defective eyes must therefore increase in every generation. Eventually, the eyeless condition or defective eyesight must become characteristic of the species as a whole. In this way a species may change, but not as a result of selection for an advantageous character. In fact, it changes because of the absence of such selection.

Now, if we accept this straightforward explanation of the fact that cave animals are optically defective, we cannot stop there. We know that Trilobites have compound eyes; that Limulus, the most archaic living Arthropod, has compound eyes; that most Insects have compound eyes; that many Crustacea have compound eyes; and that

one Myriapod genus, *Scutigera*, which is not cryptozoic[1] like other myriapods, also has compound eyes. We know also that land Arachnids, which are usually cryptozoic, have no compound eyes; that the cryptozoic wingless insects have no compound eyes; and that the cryptozoic myriapods likewise have no compound eyes. What is true of cave animals is therefore true of cryptozoic arthropods. So the differentiation of species by the survival of mutants which have useless characteristics is not confined to cave animals. When a species gets into a new environment it encounters circumstances of which some make new demands on the possibility of its survival, and others relax previous demands, i.e. thereby favour a new potential of *non-adaptive* change by mutation pressure.

Thus it is not merely true to say that Darwin's theory when rehashed in terms which would have been unintelligible to its author fails to give a comprehensive picture of all circumstances which promote species formation in the same habitat. Natural selection in Darwin's sense of the term is only half of the story which accounts for species formation in different habitats. In short, adaptive and non-adaptive differentiation are inseparably coextensive processes.

This prompts us to ask how far variations of the frequency of mutation of useless characteristics contributes to the tempo of evolution. Although we now know some physical agencies, such as temperature and X-rays, which increase the frequency of mutation in the laboratory, we do not yet know very much about the frequency of mutations in nature. One thing we do know with certainty is that some animal types such as the lamp-shell *Lingula* or the king crab *Limulus* have remained visibly unchanged over vast periods of geological time. This suggests the possibility that direct action of environment on the frequency of mutation varies considerably in different species, and therefore contributes significantly to the relative speed of evolutionary change, regardless of so-called *natural selection*. What Darwin meant thereby now implies: (a) that as species multiply, they migrate further afield, (b) that they encounter different local conditions more or less propitious to the fertility or mortality of one or another mutant type. It is now clear that we can no longer regard selection in this sense as the exclusive agent of evolutionary change, even when we have rehabilitated Darwin's theory in the light of the particulate theory of inheritance. Other agencies may

[1] That is, lives under stones, logs, bark, leaves, or the like.

play an important role independently of it or by reinforcing its effect. To be sure, selection widens the breach, and if it is not correct to speak of selection as the sole, or even as the predominant, agent of species formation in the initial stages of differentiation, it is certain that selection explains the gaps between the grosser categories of a classificatory system.

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3

WHEWELL'S DILEMMA[1]

AN ESSAY ON SOME TAXONOMICAL CONCEPTS

IT is customary, but misleading, to speak of astronomy, physics and physical chemistry as the exact sciences. Indeed some folk with unintentional humour include meteorology in the same category. It is more true to say that no branch of science attains to the status of being as exact as it might be until it has undertaken the preliminary taxonomical task of sorting out what facts are relevant and what are not relevant to the end in view. If we assume that our own species, by which I mean interfertile local varieties of *Homo sapiens*, made its appearance about 30,000 years ago, we may be sure that twenty thousand years of patient observation of the night sky by hunting and food-gathering nomads elapsed before a more settled way of life made it possible to construct a calendar based on the solar year and equipped with numerical symbols for the task. In my view, the neglect of this elementary consideration during the past four decades has forced the social sciences in general, and psychology in particular, to follow a futile trail of gamesmanship in the illusion that measurement is intrinsically elevating regardless of the fruitfulness of what one is measuring. A single irresponsible and somewhat silly aphorism of the late Lord Kelvin has made measurement intrinsically respectable regardless of relevance, the more so if upholstered with an elegant sufficiency of irrelevant algebra.

Meanwhile, there has been a spate of literature on the scientific method, i.e. what attitudes to inquiry are respectable, and whence, a term unexceptionable only if employed in the plural. In a surfeit

[1] A Farewell Address delivered in the Department of Zoology of the University of Birmingham on the eve of the writer's retirement from the Chair of Medical Statistics.

of publications by such authors as Eddington, Dingle and Woodger, we fail to find even a passing reference to a theme to which Whewell devoted so much space in the *History of the Inductive Sciences*, first published in 1857. At one time Master of Trinity College, Cambridge, Whewell has therein named after him a court in which I myself once resided briefly; but I did not make the acquaintance of his monumental and instructive two-volume treatise till about the time when I reached my sixtieth year, then on the advice of my brother-in-law, himself oddly enough a professional mathematician.

The theme of a large section of the *History of the Inductive Sciences* is: what does one mean by a *natural*, in contradistinction to an *artificial*, classification? The question anticipates one of my happiest memories as a Professor of Zoology, when I devoted the final term of the Honours curriculum to a revision of animal systematics in a course on the semantics of taxonomy. However, the issues raised have a far wider relevance—in medicine and in the social sciences, no less than in a course of zoology or of botany. Whewell's major preoccupation is indeed a dilemma which worries most students of zoology, botany and medicine. His own treatment of the issue was both highly informative and wholly inconclusive. Since then, so far as I know, nobody has done justice to the dilemma which he so lucidly exposed.

What subsumes the core of the dilemma is a question which many have asked me, when (if not so bright) they have ruminated over several text-books of zoology, or (if brighter) have had recourse to a still extant, though more than century-old, monograph on British Hydroids or Nudibranchs for a summer course at a marine laboratory. The question is: but which is the right classification? Before proceeding to do it justice, as I hope to do in what follows, I must remove one source of possible misunderstanding. Zoologists and botanists use the word *class* for a particular category of forms in a hierarchical scheme of classification, including as sub-groups orders, families, species, and included in sub-phyla, phyla and so forth. Logicians use the term *class* in a sense nearer to what we sometimes mean by classifying, i.e. any assemblage of objects with uniquely common attributes. Because there is no other current term, I shall use *class* in a sense more akin to that of the logician, but also more general, i.e. for an assemblage of related objects; and the keynote of what follows is what one means in a biological context

by *related*. From a semantic viewpoint, let me emphatically state in advance that I do not here use the word *related* in a sense itself necessarily related to a phylogeny *en rapport* with the doctrine of organic evolution. To do so would be to confuse what one means by *definition* with what one means by *interpretation*.

You will notice that I have here used the word *object* in contradistinction to *organism*. That is because we cannot begin to understand Whewell's dilemma unless we also recognize as such what was by far the most fundamental contribution of Linnaeus and his imitators, such as Cullen who was a pioneer of the taxonomy of disease. What makes the *Systema Naturae* most unique in retrospect is the systematic elaboration of a vocabulary of definition which Linnaeus invented, or at least painted on a broader canvas than his predecessors (notably Ray), in short a classification of classificatory criteria. For instance, he introduced such now familiar terms for describing leaf shape as linear, lanceolate, sagittate, petiolate *och så vidare* as they say in his own vernacular. Thereby, he raised what was essentially Whewell's dilemma at a more fundamental level than my previous remarks might imply.

Our criteria for classifying organisms in the domain of systematic botany or zoology are organs, and our characterization of a class of organisms is meaningful only if we start with a clear recognition that an organ is itself a class of putatively related objects. On the kind of relationship we predicate of the latter depends anything we can usefully say in a definitive sense about the groups of animals or plants whose relationship is our theme. In this context, *putatively* is the operative word, as a few examples will illustrate. One speaks of the *ctenidium* (when present) as a highly characteristic feature of Molluscs; but in what sense is the ctenidium of an oyster a member of the same class of related objects as the ctenidium of an octopus? One speaks of the Icelandic *Thing* as the oldest Parliament in Europe; but in what sense then does the classificatory category *Parliament* also include that of Sierra Leone? With an eye on the examiner's jargon, one may learn in medicine to specify a characteristic attribute of a syndrome as a strawberry red rash; but what is the class of all things which do in fact, in any intelligible recognizable sense, share the common attribute of being strawberry red?

Such questions invite *en passant* a few words of comment on an issue which will emerge later. To understand the viewpoint of Ray,

of Linnaeus, of Cullen or of Cuvier, whence also to get behind the Whewell dilemma, we have to go back to the twelfth-century theological controversies of the Sorbonne between Realists and Nominalists. Crudely speaking, the Realists declared that if our local language accommodates a word such as *beauty*, there must be something somewhere to which there corresponds its prototype, and, if nowhere else, at least in the mysterious Platonic heaven of universals. The Nominalist party line was, and once more crudely speaking, that nothing exists if not definable through the verbal medium. Needless to say, all of us who are scientific workers are nearer to the Nominalist viewpoint. From Bacon to John Stuart Mill, it has had too many expositors to justify my own intervention, if there were nothing more to say.

For the following reason which discloses a fundamental principle of—in the plural—the scientific, i.e. intellectually respectable, methods, I believe there is indeed far more to say. My colleagues in the medical faculty will endorse this, though perhaps they would be hesitant to attempt to formulate a verbal definition which it is easy to convey by a parable on one assumption, i.e. that all of my audience know that the British Museum of Natural History in South Kensington has a gallery which exhibits all species of British birds, duly labelled. Accordingly, imagine that we have substituted for each label a code number, and that we have invited all the farmers of Britain to identify by its code number a *cock robin*. Most of you will agree with two propositions. All would cite the correct code number; but few, if any at all, could confer on a cock robin a competent verbal specification wholly definitive from the viewpoint of a zoologist. Thus the final criterion of semantic rectitude in the taxonomic domain is not verbal definition.

To use the idiom of the Who-dun-it, we rely in the last resort on an *identity parade*, alias our old friend clinical judgement or, in one of Whewell's least felicitous asides, 'the latent instinct of the naturalist'. However, this is not to say that we want zoology to be bogged down in bird watching or that we wish medicine to shun the final arbitrament of a laboratory test. It is merely a preliminary to the recognition that taxonomy, which must always be a fundamental basis for the medical disciplines, pure and applied, need be impaled on neither horn of Whewell's dilemma. We can indeed hold firmly on both horns of a manageable bull escort. In that hope, let us now

return to what Whewell grasped firmly and to what baffled both him and his immediate successors.

I have stated Whewell's preoccupation as an attempt to answer a question which also baffles undergraduates. As I myself think, it would be fair to say that Whewell, Linnaeus and Ray—though less certainly Cullen—at least knew part of the correct answer to it, though they might not have expressed it as bluntly as I now do. Clasifications are not right or wrong. They are more or less relevant and practicable *vis-à-vis* the end in view. Needless to say, the practical end in view is in one sense identification; but to say so butters few parsnips. From one viewpoint, you and I, like Paul Robeson and President Kennedy, Macmillan and Gaitskell, Nasser, Ben-Gurion and Chiang Kai-shek, are all members of the interfertile local varieties of *Homo sapiens*; but any of us is assignable to a class which would include no one of the rest, if our main taxonomical objective is to pigeon-hole people in compartments referable to attributes as trivial, albeit real, as dermal pigmentation or religious convictions.

If we may plausibly, and at least charitably, assume that Whewell did indeed implicitly recognize this simple issue, one reason why none of his successors followed the same trial is that the theological temper of the Darwinian controversy confused every issue relevant to a realistic rationale of taxonomical procedures to the lasting mystification of all subsequent generations of students of zoology. Whether by reflection or not, the evangelists of Organic Evolution propagated the delusion both that there is such a thing as a true, in contradistinction to a useful, taxonomy of living beings and that the true end in view is to exhibit the family pedigrees of animals and plants.

To be sure, the new gospel then exerted less influence on botanists than on zoologists. In the time of Linnaeus, commercial seed production and importation of ornamental shrubs were well on the wing. Thus the fundamental problem of identification for plant taxonomists had a firm foothold in human practice long before the Darwinian doctrine diverted interest into a theological debate with little relevance to plants as such. When it did so, the alleged emergence of the human species by an exploit of thoracic surgery on our supposititious forefather Adam did not therefore intrude unduly into the serious business of the botanist. After all, everyone of us knows

well enough that the phyletic relationship between at least some Ascomycetes and Rhodophyceae or between some Phycomycetes and Green Algae is as plausible as the relationship between the Dodder plant and other Convolvulaceae. None the less, and for many good reasons, we continue to talk about Algae and Fungi—*inter alia* because we need different laboratory equipment for a Reader of Mycology and a lecturer on other groups of Thallophytes. Save in such fields as medical and agricultural entomology or helminthology, zoology is not even today so down to earth in this sense.

Aside from the consideration that an all embracing phyletic criterion of a good classification has no practical value as such, it is misleading and harmful for several reasons. In the absence of palaeontological evidence, it is a delusion to suppose that we can ever hope to be sure that *Balanoglossus* (whence possibly the Nemertinea), an Annelid or the Chaetognatha, so picturesquely (albeit superficially) like *Amphioxus*, are on the direct line of vertebrate descent. Indeed, one could keep going a series of Hibbert Lectures for a generation to dispute such hypotheses which the eminent physiologist Gaskell laughed out of court by making the best possible case for fathering on Vertebrates so unlikely an ancestor as *Limulus*. As a legal plea, Gaskell's is at least as convincing to an impartial jury as that of any counsel for other litigants.

To be sure, we can follow the historic panorama at a few levels with some assurance, but only when we have ample fossil material at our disposal. I am not sufficiently *au fait* with the latest advances of palaeo-botany to know how far we can now plausibly assign the several categories of Gymnosperms to different ancestors among seed-bearing ferns, horsetails and lycopodiums, how far we can be sure about the common origin of the flowering plants through the Cycadaceae to the Magnoliaceae, nor indeed whether the Amentiflorae are from a different stem. Admittedly, we can trace the history of Arthropods and Vertebrates with considerable confidence at the highest levels of a taxonomy tailored to the immediately post-Darwinian ideal; but there is a limit to how far we can pursue the task with rational confidence as we approach the terminal twigs of a classificatory system.

A sufficient reason for this emerges from comparison of chromosome maps of related species. We now know that in close-related species, such as *Drosophila melanogaster* and *Drosophila simulans*,

mutations at exactly the same loci may modify the wild type soma in exactly the same way. It is likewise true that mutations at several different loci in one and the same species can evoke visibly equivalent somatic effects. Thus it would be easy to produce in the laboratory from different species of *Drosophila* two pure strains each of which share more than a dozen somatic peculiarities absent in nature among all other species of the genus. If sent specimens of the two, a Darwinian taxonomist with no back-stage view of the experiment would erect a sub-genus to accommodate a suppositious common ancestor with such a unique constellation of marker characteristics. The supposition itself would be manifestly false.

For this reason alone, recognition of a natural group, no less than that of an artificial assemblage, does not necessarily imply community of descent. If the end in view is to identify a living Arthropod, the erection of a class Myriapoda is invaluable as a time-saving device, though it is highly probable that the class arose from at least three widely different aquatic ancestors. If the end in view is to identify a living Vertebrate, the division of Vertebrates into mammals, birds, reptiles, Amphibia, fishes, Cyclostomes and Amphioxus is appropriate, because highly economical and definitively explicit. None the less, such a sevenfold division is wholly inconsistent with the wealth of information now at our disposal when the end in view is to exhibit the phyletic relationship between extant living beings and the now rich reservoir of fossil remains. In terms of the latter, what we know about the multiple origin of paired fins makes the separation of Cyclostomes from fishes senseless, and mammals should appear as an order or class of terrestrial forms on all fours with Tortoises. Needless to say, this approach to the serious business of classification has no relevance whatever to the task of a Medical Entomologist in Nigeria. His (or her) main business is to decide whether a particular species of Diptera is one which can or cannot transmit onchocerciasis, if also labelled as one of the genus Simulium, likewise within the group which we colloquially call Tse-tse, as a species which does or does not transmit human trypanosomiasis.

The self-dedication of so many of Darwin's followers to an unattainable heraldic task in the delusion that only human fatigue sets a limit to pedigree reconstruction had many baleful consequences. It set physical anthropology on a false trail. It side-tracked the interest of zoologists from a nascent enthusiasm for comparative

physiology. For reasons I shall set forth later, it made a branch of science which might have been more fruitful to the advancement of medicine and agriculture unnecessarily frustrating to a beginner at the threshold of his or her biological studies. Its aim, if intelligible, has relevance only to a museum arrangement of groups whose hard parts have left sufficient informative data to clarify a quasi-historic survey without any relevance to the world's work; and even so, all we now know about convergent evolution, both from experimental genetics and from the record of the rocks, teaches us that a classificatory system based on phyletic considerations can have no certain validity at the terminal twigs of our *systema naturae*.

What is more relevant to the issue which Whewell raised is that the main effect of Darwin's teaching on systematics was to evade it, and this was inevitable only because Darwin's followers, no less than his predecessors, failed to recognize one way in which Aristotle's teaching has cast so long and so darkening a shadow over Western science in more than one domain. To expose it as such let me remind those of you who have forgotten them what the three canons of Aristotle's logic assert:

1. The canon of *Identity* states: *whatever is, is.*
2. The canon of *Contradiction* states: *nothing both is and is not.*
3. The canon of the *Excluded Middle* states: *everything either is or is not.*

Long before the turn of the nineteenth century, professional logicians had erected on the flimsy foundations of these three puns an impressive superstructure which derives its plausibility solely from the habit of speaking of *be* as an irregular verb, in contradistinction to a composite verb on all fours with *I go, I went* or *je vais, j'allais*. Since everything in the three foregoing assertions hangs on the one word *is*, it is important to emphasize four facts in the taxonomical domain of grammar and semantics:

(a) in all languages of the Indo-European family several of about nine roots, usually at least three, and in English four, go to the making of this mixed bag *be*;
(b) rarely more than two are common to any pair, if not closely related;

(c) we can trace the semantic origins of some such roots to different sources;

(d) what is even more relevant to our theme is that the uses of the several components are in some languages semantically distinct.

One might list at least seven different meanings of what we convey by *is* in English or *sum* in that spuriously profound revelation of Descartes, *cognito ergo sum.* Only two are relevant to our theme. It would be impossible to translate the Cartesian caption into Japanese and thence back into English without exposing its idiocy; and it is impossible to translate the Aristotelian canons into the emphatic affirmative Welsh construction without detecting a semantic ambiguity whose significance did not become apparent to mathematical logicians before the beginning of our own century. Here are two examples:

JOHN YW EIN MEDDYG *John is our doctor*
JOHN SYDD YN FEDDYG *John is a doctor*

The first statement in which we translate *is* by YW (≡ as used by many mathematicians) is equivalent to saying that *John* and *our doctor* are different labels for the same object; and we may speak of this usage as *the be of identification.* The second statement in which we translate *is* by SYDD (nearer to =) is equivalent to stating that John is one of the class of adults whose common property is a medical qualification; and we may speak of this usage as *the be of predication.* The distinction is vital. If we are talking about the *be* of identification, Aristotle's three canons are tautologies. When we are talking about the *be* of predication, they place an avoidably tiresome burden on discourse, if only because they restrict our claim to label an object as of a class to a particular instant on a time scale, as when we say that the notochord *is* a common property of Vertebrates.

Aristotle's canons lead to the class concept of what we nowadays call a *two-valued* logic. In this system A, B and C constitute a class in its own right if *all* three possess at least one attribute not present in the residual universe. In contradistinction to such a binary (i.e. Aristotelian) class we can, and more commonly do, assemble objects of biological interest in accordance with either of two other criteria. The first of these I used to call a *nebular* class when lecturing to

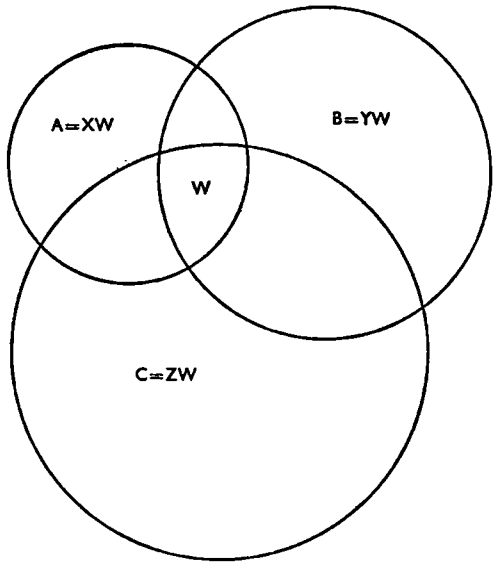
final honours students of zoology. I now prefer to speak of it more informatively as a *catenary* class, meaning that A, B and C constitute such a class if: (i) A and C each share with B at least one attribute absent in the residual universe; (ii) A and C share no such attribute.

In systematic botany or zoology these two categories suffice for one or other end in view; but in diagnostic medicine a third class concept emerges. This we recognize as valid in practice by appeal to the principle of the identity parade, but we may rationalize it by recourse to one of two criteria, and on that account I shall speak of it as a *quasi-statistical* class. In it, we assemble objects in terms of attributes which may indeed be present in the residual universe; and we do so justifiably for one of two reasons. In such syndromes as Mongolian Idiocy or the Lawrence-Moon-Biedl constellation, the probability of finding any two such attributes, and *a fortiori* more than two, in the same object of the residual universe would be very minute if we were to assume a random distribution of all relevant attributes. Alternatively, as when we speak of a *sub-clinical* case during an epidemic of poliomyelitis or infective hepatitis, the category is meaningful in so far as the frequency of its occurrence at a restricted period of time vastly exceeds what we should expect on a comparable assumption. So far as I am aware, no one has hitherto ventured to verbalize such classificatory concepts as the syndrome and the sub-clinical case in terms as explicit as the foregoing. Thus the third formulation of a class concept is irrelevant to Whewell's dilemma.

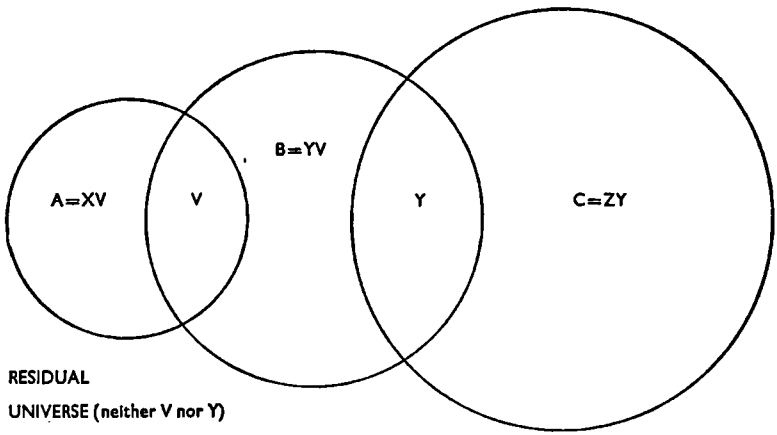
By now, my discerning audience will have anticipated that the binary-catenary dichotomy of my own idiom is very close to the traditional dichotomy of artificial and natural taxonomy. To clarify, and to dispose of, a dilemma that need not be, let me therefore now make two categorical assertions:

(a) if the end in view is to identify an object, our *be* is the *be* of identification, whence we necessarily invoke a binary procedure of progressive exclusion, as indeed we do when we use a *key* to genera or species;

(b) any such procedure would be incredibly time-consuming unless we could initially narrow the field of observation by going, so to say, to an appropriate set of library shelves, to the right museum floor or to the relevant filing cabinet at the start.



RESIDUAL UNIVERSE (Not W)



RESIDUAL
UNIVERSE (neither V nor Y)

Inescapably therefore, a useful taxonomy of animals or of plants has to accommodate two aims which come into conflict only if we fail to recognize at what levels of a taxonomical hierarchy one or other is appropriate to the delimitation of the class. One is precision of definition in the final stages of identification by procedures which are, and must be, *en rapport* with the Aristotelian class concept of a two-valued logic of identification. The other is the need to shorten our task by arranging our objects with a view to *storage* of as much information as the human memory can deal with and as effortlessly as need be. At least implicitly, we then invoke the catenary class concept; and this is necessarily so for a simple reason.

In the simplest conceivable situation, a catenary class, as here defined, embraces at least two maximal binary classes and in practice, if a useful assemblage, will embrace an immensely large number. This signifies that a tidy binary classification involves vastly more hierarchical categories and accordingly imposes on us a much greater *mnemonic load.* Hence the more comprehensive we make a classification which is a serviceable key, the less resemblance will it have to what we call a natural classification; but we cannot dispense with the artificiality which the *be* of identification imposes on the definition of our classes as we approach more nearly to the end of the journey. In practice therefore we compromise.

What economy of storage is at worst and at best achievable for a given number of relevant attributes and objects is reducible to an exact calculus. If a catenary assemblage contains N objects each of which is a unique combination of x out of y attributes not present in the residual universe, it is always possible to arrange them in one or more dimensions as a sequence or nexus such that each member shares at least one attribute with each member adjacent to it. What then emerges is that we need to be familiar with very few members of the class to face with confidence an identity parade in which our assignment is to label any other member as such; and a precise measure of the advantage of a natural class is the ratio of the minimal number of objects with which we then need to be familiar to the total number of class members. We can make more or less parsimonious assumptions about its structure. *Ceteris paribus*, the most parsimonious, i.e. that which involves least economy of mnemonic effort, is that each member shares no more than one attribute with some other; but I shall here exhibit for heuristic

interest a situation (Table 1) in which each member shares two.

TABLE 1

Ten definitive attributes, each object a unique combination of three; every possible three-fold combination 120; binary classes nine, each with only one common characteristic.

1	ABC	BCD	CDE	DEF	EFG	FGH	GHJ	HJK
2	ABD	BCE	CDF	DEG	EFH	FGJ	GHK	
3	ABE	BCF	CDG	DEH	EFJ	FGK	GJK	
4	ABF	BCG	CDH	DEJ	EFK	FHJ		
5	ABG	BCH	CDJ	DEK	EGH	FHK		
6	ABH	BCJ	CDK	DFG	EGJ	FJK		
7	ABJ	BCK	CEF	DFH	EGK			
8	ABK	BDE	CEG	DFJ	EHJ			
9	ACD	BDF	CEH	DFK	EHK			
10	ACE	BDG	CEJ	DGH	EJK			
11	ACF	BDH	CEK	DGJ				
12	ACG	BDJ	CFG	DGK				
13	ACH	BDK	CFH	DHJ				
14	ACJ	BEF	CFJ	DHK				
15	ACK	BEG	CFK	DJK				
16	ADE	BEH	CGH					
17	ADF	BEJ	CGJ					
18	ADG	BEK	CGK					
19	ADH	BFG	CHJ					
20	ADJ	BFH	CHK					
21	ADK	BFJ	CJK					
22	AEF	BFK						
23	AEG	BGH						
24	AEH	BGJ						
25	AEJ	BGK						
26	AEK	BHJ						
27	AFG	BHK						
28	AFH	BJK						
29	AFJ							
30	AFK							
31	AGH							
32	AGJ							
33	AGK							
34	AHJ							
35	AHK							
36	AJK							

Here we have a model situation in which we assume:

(a) the class members (i.e. objects) may contain one or other of ten attributes not present in the residual universe;
(b) each object is specifiable by recourse to a unique combination of exactly three of them.

If we make the assumption that every such combination has a representative, the class will contain 120 objects and is divisible into nine binary classes, all members of which have at least one common attribute. Evidently therefore we need to be familiar with only nine of them in order to identify any of the remaining 111 as class members. Now we can arrange these nine classes, defined arbitrarily as we have here done, so that the first row consists of members in an ordered catenary sequence in which it suffices to identify the nine members by only two objects labelled CDE and FGH, involving six attributes in all (C, D, E and F, G, H). The catenary nexus remains if we eliminate all objects which are not specifiable by recourse to at least one of these six. In fact, this means that we reject only four, viz. ABJ, ABK, AJK, BJK. Whence familiarity with two objects suffices to identify 116 in all. Since our four rejects all share J, K or both, they are in catenary relation to one another and to any residual members with the same characteristic. Hence three objects in all suffice to recognize all the 120-fold assemblage as such.

I shall later refer to such objects as transitional types, and emphasize *en passant* that *we know a catenary class, if we know the minimum number of its transitional types*. What is no less important to stress in this context is that we can investigate three conditions which limit our index of storage economy:

1. we might impose the condition that no member of the class shares with any other more than one common attribute; and this, as stated, is the most parsimonious limit to what we can achieve by a natural classification;
2. we might presuppose that the members share at least three; and this would make each column longer in conformity with an elementary formula of the combinatory calculus, so that our two transitional types would suffice for recognition of a class substantially larger than 116;

3. we might presume that any member could share with any other 1, 2, 3 up to 9 of the 10 attributes, and this would make our table exhibit for only 3 transitional types an immensely larger class of objects.

We are thus able to dispose of Whewell's dilemma in a way which is respectable from the viewpoint of modern logicians. In terms of the principle of the identity parade, i.e. what Whewell calls the latent naturalist instinct, common experience during the century before Linnaeus wrote had led to recognition of large assemblages of seemingly related forms. At a verbal level of definition, however, there was as yet no room to accommodate a hierarchy of classes in a respectable[1] way except by recourse to definitive terms which inescapably rely on binary exclusion. Such is the core of Whewell's dilemma, and with one notable exception no naturalist of the eighteenth century got to grips with it. I here refer to his *Familles de Plantes* in which Adanson published in 1763 what he called the *universal method.* He tackled the issue with a lucidity which brought sharply into focus the impossibility of harmonizing the denomination of so-called natural assemblages with the Aristotelian class concept. I quote from Whewell:

> Considering each organ by itself, he formed, by pursuing its various modifications, a system of division in which he arranged all known species according to that organ alone. Doing the same for another organ, and another, and for so many, he constructed a collection of systems of arrangement, each artificial—each founded upon one assumed organ. The species which come together in all these systems are, of all, naturally the nearest to each other; those which are separated in a few of the systems, but contiguous in the greatest number, are naturally near; those which are separated in greatest number are further removed from each other in nature; and they are the more removed the fewer are the systems in which they are associated.

I myself have not consulted the original text, concerning which (says Whewell) Cuvier remarks that the author 'conceived himself thus empowered to reach far beyond the strength and the lot of man'.

[1] See remarks by Jevons (p. 695 of *The Principles of Science*) concerning the botanist George Bentham's views (1827) on the logic of classification.

Like his successors including Whewell, Cuvier here failed to detect the cardinal error, i.e. that no human ingenuity can reduce a natural assemblage conceived in terms of catenary class relationship to a precise definition referable to a binary classification of all its attributes. Indeed, neither Whewell nor Darwin, whose remarks on the Crustacea in the *Origin* so clearly pinpoint what at least a natural assemblage is *not*, ever came to grips with the issue.

In these days, i.e. since 1940, professional philosophers recognize the validity of multi-valued logic, and even (on somewhat dubious assumptions) stochastic logic which can accommodate a so essential concept of medical diagnosis as the *quasi-statistical* class. Thus the foregoing disposal of Whewell's dilemma might seem somewhat jejune to anyone who is not familiar with the unnecessary obstacles which confront a student making his or her first acquaintance with systematics in a university course of zoology. I do not hesitate to devote some of your time to clarifying their sources, because I believe that familiarity with a wide range of animal forms is an essential prerequisite to the continued progress of physiological science.

Preoccupied with gadgets and accustomed to rely on the laboratory animal, too few physiologists nowadays realize the importance of preserving a niche for systematics in a curriculum of biological studies. I may therefore pardonably justify my last assertion. Circumstances peculiar to the organization of individual species make it possible to get the sort of clues to fruitful hypotheses which we could scarcely hope to formulate if we set our sights exclusively on a limited range of laboratory stock such as the white rat, or still less *Homo sapiens*. For instance, the facts that *Drosophila* has only four pairs of highly heteromorphic chromosomes, a life cycle of ten days in a highly standardized culture medium of yeast in agar at controlled humidity and at a fixed incubator temperature, that we can mate it at will and that it produces several hundred offspring within a few hours made it possible within twenty years to elaborate a picture of genetic architecture which we could never possibly hope to do in a millennium if our only material happened to be the cow. Though we cannot cross human beings at will or maintain them under standard environmental conditions, though the size of the human family is small and the length of a human generation is about 1,000 times that of the fruitfly, though also the human species has forty-six pairs of chromosomes, we can now devise ways and means of showing how

much of what we have learned from *Drosophila* is relevant to the nature of Man. Without this initial impetus, we could not conceivably have hoped in the course of many millennia to have learned what we have now learned about human inheritance in half a century.

In a different field of experimental inquiry, consider an anatomical oddity, with the significance of which every first year student of zoology should be—though rarely is—familiar. In the frog, as in other Amphibia, the renal tubules receive their blood supply from the renal portal vein and Bowman's capsule from the renal artery. By recourse to the simple device of ligating one or other, this makes it possible to observe the outcome when we put either tubule or capsule out of circulation. So simple an experiment, long since forgotten by many, laid the foundations of modern renal physiology.

It must thus be clear why a knowledge of systematics should be the heritage of every research worker in the medical sciences, pure and applied; and if so, it is important to reduce time spent in acquiring such knowledge to manageable dimensions. None the less, it is still true to say that every extant text-book of animal systematics, such as the Sedgewick treatise or that of Parker and Haswell as lately revised, prolongs the period of initiation unduly by perpetuating three logical errors, which the foregoing excursions into Aristotle's two-valued logic should have made manifest as such.

The first is that a catenary class can ever be definable by a binary specification of its attributes, i.e. by reference to characteristics common to all of its members. Long ago, Darwin at least implicitly recognized this truism in his remarks already mentioned; but the error is still prevalent. Thus it is not uncommon to find in a current text-book at the beginning of the section on so good a catenary group as the Coelenterates words to the effect that they have a body with a single cavity having one orifice (a statement which is not true of *Ctenophores*); two cellular layers (a statement which is true with some qualifications of *Hydroids* only); and that a mesogloeal jelly separates these two layers (a statement which is not convincingly applicable to most *Hydroids*). Here we have a series of assertions based on a binary classification of attributes, each true of some, but not intelligibly true of all, Coelenterates. Similarly, we still meet in ostensibly reputable text-books statements such as that Molluscs have a body with three divisions, a head, a mantle and a foot. The first part of this assertion is false as applied to Pelecypods and

Scaphopods. The second is not obviously intelligible as applied to the snail. The third is not obviously intelligible as applied to the Squid.

Like the Coelenterate mesogloea, the Molluscan foot is not a satisfactory definitive term, and we here come to grips with a second source of mystification which unnecessarily bogs down the beginner. If it is inconsistent with its definition to specify a natural, i.e. catenary, group in the binary terms which are essential for construction of a key, it is equally true that it is meaningless to define a catenary group by reference to characteristics which are themselves catenary classes. We do so, if we convey the glad news that the *ctenidium* is a highly characteristic Molluscan structure. Assuredly, it would be difficult to recognize any intelligible relationship between two organs so different as the gill of an oyster and the gill of a squid, unless we could trace what we name as such through a catenary nexus of forms such as Chiton, Nucula or Yoldia, Fissurella, the whelk and the Pearly Nautilus. The ctenidium is a catenary class of objects, as also is Parliament in the otherwise semantically and logically unintelligible assertion that the Icelandic Parliament is the oldest in the world. In short, it is not true to say that we recognize a Mollusc as such when it has a ctenidium. It would be nearer to the truth to say that we recognize a ctenidium as such by the fact that its owner is a Mollusc.

That we cannot intelligibly define a catenary class of species in terms of the common possession of an organ which is itself a catenary category does not, however, mean the same thing as saying that we first need to know all Molluscs before we can recognize a Mollusc as such. To say that a group is a good natural group in the catenary sense signifies that we can at sight identify a relatively large assemblage of forms as such if we are familiar with comparatively few *transitional types*, i.e. the minimum number which conform to the catenary criterion that every member of the class must share with at least one other member some attribute absent in the residual universe. This means that we should replace all formal definitions by demonstrations of what are the transitional types; but, if so, we encounter a third source of mystery-mongering in widely current teaching. Assuredly, what the type system of Thomas Henry Huxley, followed by so many text-books such as that of Parker and Haswill, does not do is to put transitional types into the foreground

of the picture. Those of us who have a wide acquaintance with the Mollusca agree that it is a peculiarly well-assorted natural assemblage; but one could choose few three animals less transitional, and accordingly less likely to enlist the confidence of a beginner, than the pond mussel, the garden snail and the squid.

It should now be clear that Whewell's dilemma, unresolved by Darwin's followers, arises merely from an initial failure to recognize the limitations of Aristotle's two-valued logic now widely repudiated by mathematical logicians. Having recognized its limitations, we see clearly that different criteria of the credentials of a taxonomy are applicable to its several uses and what impediments may arise from the attempt to impose the same criteria on a taxonomic system devised, like the W.H.O. diagnostic code of diseases, with more than one end in view. Justifiably so, botanists are perhaps less fastidious than zoologists about sacrificing the demands of identification to those of storage; but this is not necessarily harmful if one knows what one is doing.

The foregoing remarks refer primarily to the classification of organisms in contradistinction to that of organs. Both botanists and zoologists recognize in the latter context a useful distinction; but some zoologists at least pay lip service to another which is somewhat silly. The useful distinction I refer to is between analogy and homology. This, as you all know, is the difference between classifying the parts of an organism respectively by their uses or by their structure and mode of development; and it would sidestep some futile controversy about the teaching of elementary grammar if linguists were more alert to a similar distinction in their own field.

When we classify German words as pronouns, verbs, nouns, adjectives and particles we do so at the level of homology, i.e. structural resemblance in terms of the terminals a stem can take, or what internal vowel changes it may undergo. When we classify English words in a far less clear-cut way as pronouns, verbs, substantives (i.e. noun or adjective equivalents), adjectival adverbs and particles, the same is true. On the other hand, the distinction between *during*, *while* and *meanwhile* with exactly the same semantic content as respectively preposition, conjunction and conjunctive adverb refers only to their uses. In short, homology corresponds to the domain of accidence, analogy to the domain of syntax, and

the grammar of school books of the twenties was a hotchpotch now recognized as such but not alas as yet widely why so.

A distinction which is less, if at all, meaningful, is one proposed by Ray Lankester (1875) in the heyday of profitless phyletic speculation. He[1] distinguished, and others have followed him, two kinds of homology. These he called 'homogenetic and homoplastic agreements'. Briefly, Lankester said that an organ of two individuals was homogenetic if the ancestor of both possessed it in the sense that the pentadactyl forelimb of Sauripterus falls into a catenary sequence with the forelimb of the one-toed cart-horse or donkey. If not, not; whence homoplastic. At even an elementary level, we can here suspect a leg-pull. Any second year university student of zoology knows that tracheae have turned up several times in the Arthropoda; and the reflective student will have asked the question: what is there about the genetic architecture of Arthropods which encourages tracheae to turn up? A third-year student knows both that the one-toed ungulate turned up also in the Litopterna and that the common ancestor of the donkey and the cart-horse probably was three-toed. In short, the distinction is impracticable, even if we indulge in the quaintly impracticable fantasy that we can ever hope with certitude to embody our exercises in phyletic speculation in the terminal twigs of a pedigree.

At a more sophisticated level, the distinction is silly for the following reason. Shortly before World War II, Gordon and Sang discovered the *Antennaless* mutant of *Drosophila* in my laboratory at Aberdeen; and demonstrated that the gene *Antennaless* suppresses the antennae on an insufficient diet of one of the B-vitamin complex. Having established a pure strain of mutant stock, they were able at will to hatch out flies with or without antennae by varying their diet appropriately. Thus the common ancestor of two individuals with antennae might be an individual without them. In that event, were their antennae homogenetic or homoplastic from Lankester's viewpoint? To ask the question is to answer it in the sense that the question is meaningless. Here Lankester perpetuated an elementary prevalent confusion still current when even those who recognized the validity of Weismann's challenge thought in terms of inheriting characters as such instead of thinking in terms of our only link with our ancestors through what Weismann himself called the *germ track*.

[1] *Ann. Mag. Nat. Hist.*, VI, 1870, pp. 34–43.

Gordon and Sang found evidence (unpublished) of a gene that suppresses the Antennaless gene, thus reinstating the *status quo.* This sort of thing may well happen again and again in a period of evolutionary transition. About the same time I found in the Museum collection of Aberdeen an armadillo skull with what one would call a post-orbital bone if the possessor had been one of its Theromorph ancestors. In correspondence with D. M. S. Watson and Broom, whom I had known so well in South Africa, I found that this bone, otherwise missing in the Mammalian skull, does in fact turn up at times in Armadilloes. In the light of the well established possibility of back mutation and the existence of modifiers which suppress a suppressor, it is not amazing to find an authentically post-orbital bone in an authentically contemporary armadillo.

Some who read what has gone before may wonder why I have chosen so antiquated a topic for my farewell address to the Faculty. I have done so because I believe that day to day familiarity of the naturalist and of the clinician with the raw materials of their several disciplines remain the meat of biological science. Statistical theory is not a safe recipe for usefully sidestepping the serious business of deciding what you are classifying. All the medical sciences owe a continuing debt to the sort of knowledge which familiarizes us with a wide range of organic types; and the future of physiology still depends on knowing the right animal or plant to provide the right clue to the right problem. Therefore it remains a worthy activity to know widely about creatures not as yet domiciled in cages, and to know them rightly we must think deeply about how to label the cages in which we put them. It is a grievous heresy to believe that a science can run before it has learned to toddle. It may be that this heresy is holding back the progress of psychology and sociology today.

4

THE NATURE-NURTURE ISSUE IN A CONTEMPORARY CURRICULUM OF MEDICAL STUDIES[1]

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AT the lunatic fringe of biology there is a body of doctrine called *eugenics* with which I suppose most people associate any claims of genetical enquiry to a useful niche in medical science. A British Eugenics Society in the thirties drew its support largely from a vocal minority of adherents to the Nazi ideology of *Rassenhygiene*. Even now, Professor Darlington has emerged in our midst as a protagonist of the same viewpoint, unscathed by a war which ended with ritual sacrifice of the redoubtable and unlamented Alfred Rosenberg.

The theme of what follows is that genetical enquiry can make a considerable, and as yet unmade, contribution to medical science, that its major contribution as I see it has very little to do with what we commonly understand by eugenics, even if pruned of racial prepossessions, and that it will not be possible to assess how important it is unless we recognize how exiguous are the claims of the eugenic viewpoint in the domain of preventive medicine. At the outset, I must therefore emphasize why it is all too easy to overstate the claims of genetics as an instrument of policy in the preventive domain. To do so, I must briefly and reluctantly recapitulate views and terms which I have elsewhere put on record.

[1] Cecil Gordon Memorial Lecture delivered in the University of Edinburgh. Nov.. 10, 1960.

[2] Much of the substance of this lecture has appeared as an article in the Journal of Medical Education, 1958, Vol. 33, p. 421, and is reprinted here by kind permission of the Editor and the Association of American Medical Colleges.

We need to retrace our steps through a quagmire of semantic ineptitudes if we hope to get the legitimate claims of genetics into focus at any level; and if we hope to discuss in a rational temper any issue involving genetic principles, we must first relinquish the luxury of misleading terms such as *familial* traits or *hereditary* characters. Traits or characters are end-products of a developmental process in which there are highly intricate reactions between an initial equipment, we may call *nature*, and extrinsic physical and social agencies, which we may collectively call *nurture*, including thereby what I ventured some thirty years ago to subsume under the epithet *uterine environment*. All one can legitimately convey by human *heredity* or *nature* in a biological, in contradistinction to a legal, context is the initial material contribution of the sperm and the ovum to the direction in which the developmental process proceeds. Accordingly, the subject matter of genetics is not a particular class of characters, but the effect on every attribute so labelled of one or other *gene substitution* in a specified nurture framework and a specified complex of all other genes.

After this preliminary dismissal of one source of mental confusion about the claims of genetics in a medical curriculum, it will also clarify the major issues for further scrutiny, if we now made a threefold split of gene-substitutions as follows. By *quasi-compulsive* gene substitutions I designate such as lead to one or more highly characteristic end-products, which we may call *marker characters*, in a range of environmental variation and genetic make-up so wide as to include most situations consistent with viability. By *extra-limited* gene substitutions I signify such as lead to one or other characteristic end-product in a restricted environmental situation, e.g. the highly instructive gene substitution which eliminates the appearance of antennae in *Drosophila* if, and only if, the culture medium contains too little of one of the B vitamin complex. By *intra-limited* gene substitutions I label those whose recognizable effect, if any, depends on the particular gene complex which we may call its bedfellow. In the human domain, I strongly suspect that the blue sclerotic-deafness-*os fragilis* and that the Lawrence-Moon-Biedl syndrome belong here. Needless to say, the last two categories are not exclusive. A gene substitution may be both *extra-limited* and *intra-limited* in my vocabulary.

What is equally important to dismiss is the notion that these two

classes are out of the ordinary. That we are too apt to think of the subject matter of genetics primarily in terms of quasi-compulsive substitutions admits of a simple enough explanation. Were there no such, it would have been well-nigh impossible to stumble on the basic principles of genetic architecture enshrined in the chromosome maps of species. Accordingly, we hear most about them in an elementary course of instruction, and the student of genetics therefore gets to think of them as the representative class, in contradistinction to the class most easy to handle. By the same token, we think of some end-product, which I have designated a *marker* characteristic, as the unique effect of the gene substitution, but for no better reason than because we score our records by reference thereto. When we elliptically speak of the sex-linked white eye gene of *Drosophila*, we should do so with the reservation that very few gene substitutions have so characteristic an influence on the developmental process in so wide a range of extrinsic variation during the developmental process and that white rather than red eye is by no means necessarily the only recognizable outcome which the same gene substitution may have in one and the same gene complex and in one and the same environmental framework.

The last remarks would be trite if experimental geneticists refrained from the use of such highly misleading terms as *modifying genes*, an expression which conveys to the neophyte the impression that some genes are modifying, others not. To be sure, one may sometimes forgivably dispense with the foregoing refinements of discourse when dealing with animal or plant genetics, because one can then deal with gene substitutions one at a time, in the Baconian way. By using closely inbred stocks, as did Mendel and the other pioneers, and by using a highly standardized regimen of culture brought to push-button level of perfection in the fruit fly saga, we can eliminate all, or nearly all, contributory variations of the gene complex as a whole or of the nurture component of the developmental process. We can do neither one nor the other when our experimental material is human; but this circumstance does not distinguish human genetics as such from all other branches of human biology. We can justly assess the claims of genetics in the medical curriculum only when we realize an essential difference between animal physiology or animal pathology on the one hand, and human physiology or human pathology on the other.

I shall not apologize for emphasizing this distinction. Our legitimate use of the term *experimental pathology* obscures the issue on one level, and our misleading use of the term human physiology more often than not for the physiology of the frog, rabbit, rat, guinea-pig, golden hamster, ferret, cat or dog confuses our thinking at another. In animal physiology, as we usually use the term in a sense which excludes genetics, and in animal pathology equally, our major preoccupation is with nurture. We do indeed, or should be able to, deal with it as such because we can partial out heredity in the contemporary idiom of statistics by recourse to genetically standardized material such as the Wistar Institute strain of white rats. Just as we cannot standardize, or at least only imperfectly and with great difficulty, the nurture component of the human developmental process for the study of human inheritance, we cannot standardize the nature component for the study of human physiology or human pathology except in so far as we restrict our observation to pairs of monozygotic twins. Because we too easily overlook the consequences of this limitation, it is all too easy to exaggerate how much we can learn about human genetics with methods at our disposal.

If we recognize it as such, it is clear that the most refined methods of analysis applied to pedigrees, and population statistics can tell us little about any class of human gene substitutions other than the class I have labelled *quasi-compulsive*. This includes two subcategories: (a) about at most forty so-called familial and hereditary conditions; (b) serological differences. I should not like to set a limit to how much further advance the immediate future will record in the domain of the second category which is not without some interest to preventive or curative medicine in terms of incompatibility risks; but I do assert with confidence that there is no immediate prospect of greatly enlarging our existing knowledge about the first. My reasons for stating so are: (a) refined statistical analysis of population data is only applicable to rare conditions; (b) a large-scale hospital survey undertaken by the M.R.C. at my suggestion in the mid-thirties failed to tell us anything noteworthy about such rare conditions other than to confirm what we already knew. The fact that the so-called familial and hereditary diseases, of which we can identify a gene substitution as the predominant aetiological component, are also rare emphasizes that their interest to preventive

medicine is trivial. If they are lethal, as is true of several, including either form of Tay Sachs disease, we must assume that selection is in equilibrium with mutation. It follows that the most rigorous enforced selection, if socially practicable, could achieve no useful outcome.

In short, it seems to me that the claims of genetics in a contemporary curriculum of medical studies are very weak if they rely only on such considerations as the eugenists advance; but my concern in this context is to put forward a different criterion of usefulness. I can do so only if I now digress to discuss the implications of the term *aetiology*, derived from the Greek word *aitia* for *cause*. As an undergraduate at Cambridge before World War I, it was my privilege to listen to Bertrand Russell when he delivered in my own college (Trinity) the course of lectures published under the title *Our Knowledge of the External World*. The one which made the most lively impression on me (also republished in *Mysticism and Logic*) had the title *The Notion of Cause*. During the first thirty years of my professional career, I never used the word *cause* in a lecture; but I have now lapsed into doing so, because the inherent danger lies less in the use of the noun than in the use of the article which precedes it. While we can never—except elliptically when the context makes the limitations of the form of words explicit—speak with propriety about *the* cause of an occurrence, we can speak of *a* cause without clouding the issue. Thus we rightly conceive its province only if we regard *aetiology* as the disclosure of a *causal nexus* in contradistinction to the identification of a *singular* cause.

In its simplest terms, a scheme which can convey an exhaustive biological approach to the elucidation of such a causal nexus will be a three-dimensional grid. Along one dimension we then conceive a layout of the several taxonomic criteria definitive of a particular syndrome. Along a second we conceive different realizable combinations of extrinsic variables, uterine and post-natal, physical and social. Along the third we must conceive all admissibly different specifications of the gene complex of an individual patient. Needless to say, we can never hope to have all the data to fill in the cell entries; but a schematization of this sort is none the less a salutary reminder that disease, dysfunction and malformations, no less than what we call *characters* of the healthy organism, are, like all attributes of a living being, end-products of an immensely intricate interplay

between the class of antecedents subsumed by the terms nature or inheritance as already defined and the class of variables subsumed by the expressions nurture or environment. Until we can indeed approach the patient in this three-dimensional way of thinking, *aetiology* must remain at every level content with half-truths through our failure to formulate our questions about disease and health in a sufficiently comprehensive way.

Meanwhile, the prepossessions of the investigator perpetuate such half-truths by indoctrination with a one-sided emphasis sometimes on nature but often on nurture. Thus, the student who attends a course on the so-called deficiency diseases commonly learns that a cereal diet exclusively of maize or of polished rice causes [*sic*] pellagra or beri-beri, as the case may be. Needless to say, many people who eat polished rice and many people who eat maize and otherwise feed like the victims of these conditions, never in fact suffer from them. Nor is this very odd. Since carnivors can do without ascorbic acid, it would not be surprising if some human genotypes can do without one or other of the B series. This being so, it is equally proper to speak *either* of failure to synthesize a particular accessory food constituent *or* of absence of the same constituent in the diet as *a* cause of a particular deficiency disease; and if one speaks loosely of one or other circumstance as *the cause*, all one can legitimately convey is that one of several different agencies or circumstances designated with equal propriety by the same word happens to be the aspect of the total situation most readily susceptible to management. The ellipsis would be innocuous were it not for the fact that we evade the challenge to a deeper understanding of the requirements of the human body by curtly dismissing all aspects of the problem outside the scope of *intended action. Mutatis mutandis*, what may be said of the literature of the dietetic deficiency diseases applies with equal cogency to most recorded information with reference to iodine shortage and the prevalence of goitres or to cancer of the lung and smoking. In saying so, I do not wish to convey that genotypic differences necessarily supply the clue to the knowledge we lack. The case history of the individual subsumes a vast range of nurture variables, which may well be as relevant to the enigmas of clinical variation. My sole concern here is to emphasize that the current tradition of interpretation may lean too heavily on the role of one or other class of causal agencies.

What is true of our customary attitude to the dietetic deficiency diseases is even more true of our approach to diseases which fall within the communicable group. In this context a three-dimensional model is no longer adequate. The causal specification of cell entries in terms of presence or absence of taxonomic criteria definitive of the syndrome must now embrace a parasite-host relationship, each component of which is free to vary independently in the two dimensions of the nature-nurture dichotomy. Plant pathologists have made some headway in elucidating the causal nexus at this level of sophistication; but scrutiny of any contemporary text-book on epidemiology discloses a vast array of questions which still await solution less because we lack the means of finding answers than because we lack the attitude of mind to ask the right questions. To be sure, advances in the genetics of micro-organisms during the present decade have provided enough clues. Meanwhile, epidemiologists continue to beatify the partnership of Topley and Greenwood for no discernible reason other than one I shall state simply in the next sentence. What Topley and Greenwood conclusively showed is that it is possible to make an animal experiment as inconclusive as a statistical inquiry on a human population only if one dismisses all Baconian principles by muddling every conceivable issue of nature and nurture at every possible level.

Some may here suggest that the considerations outlined in the foregoing remarks are of relevance to training in medical research rather than to training in the practice of medicine; but are not most students of medicine liable at some time to experience an intellectual *malaise* which is comprehensible only in such terms? In their preclinical laboratory studies, they may and should acquire confidence in accredited scientific interpretation of biological phenomena by familiarity with experiments in which it is possible to control nature in the process of varying nurture or vice versa. At the bedside, treatment and diagnosis inevitably lack the exacting tidiness of a well conducted animal experiment; and an aura of uncertainty which so often engenders lay criticism of the sort we expect to meet in a Shaw Preface is what must needs be, while we know so little about the interplay of both major categories of human variation. Because current teaching does too little to emphasize the sources of variations which only a deeper understanding of human inheritance and developmental physiology can clarify, it fails to inspire a rational

confidence which is alien both to irresponsible scepticism and to unreflective reliance on authority.

Accordingly, the plea I am advancing is that some sort of instruction in genetics is an essential ingredient of the medical curriculum less because of its direct usefulness as a guide to preventive action, such as compulsory sterilization or precautionary measures for putative parents with serum incompatibilities, than because it is an indispensable prerequisite to a comprehensively rational attitude to aetiological issues. To promote such an attitude, it is not sufficient to insert in the curriculum a few lectures on familial conditions and blood group crosses in the teaching of social medicine, nor to insist on an extra dose of Drosophilately in pre-medical courses now largely taken over by the grammar schools. Neither the one nor the other suffices to force the attention of the student on what should be the main end in view, i.e. what *antennaless* so beautifully makes manifest —in short, the realization of the immensely complex possibilities of interaction between nature and nurture at every stage in a developmental process rightly conceived as embracing the whole life cycle.

If, as is true of most British universities, a student attends a few lectures on human heredity, so-called, without a thorough grounding in animal or plant genetics, he or she will not be ready to appreciate the methodological difficulties of evaluating the role of nature in populations of organisms which we cannot mate at will, and the few who have had any training in genetic principles at the pre-medical stage will have had little acquaintance with nature except in the domain of marker characteristics whose exhibition has the double safeguard of recourse to highly standardized genetic material and to a highly standardized regimen of extrinsic agencies incident to development. A course of study in human genetics unfortified by a firm groundwork in animal and or plant genetics will accomplish little or nothing to promote the intellectual orientation which is the subject of my plea. By the same token, a course in animal or plant genetics unrelated to what experimental embryology teaches us about the possibilities of modifying the developmental process is also inadequate. From the viewpoint of medical science in terms of the plea here put forward, we need to formulate a programme of instruction in animal and plant genetics as part of a more comprehensive discipline which the Germans call *Entwicklungsmechanik*, or if you prefer it, *developmental physiology*.

Where, as in Cape Town and later in Aberdeen during the period of my most intimate association with Cecil Gordon,[1] all medical students take a first-year university course in biology, the proper place for instruction of this sort is at the beginning of their studies. Such indeed was my teaching policy in which Cecil Gordon himself collaborated enthusiastically. To the best of my knowledge, the majority of medical students in Scottish universities still take a first-year course of elementary biology which is at least amenable to such orientation. In England and Wales on the contrary, all the provincial medical schools grant exemption in *lieu* of a biology credit in the grammar school General Certificate of Education (Advanced level). Consequently, the majority of medical students plunge into the study of anatomy and human physiology with no grasp of basic biological principles.

A scrutiny of the English and Welsh scene prompts three reflections:

(a) Owing to pressure for space in the universities, we must accept the fact that the grammar schools will continue to take over the function of pre-medical biological instruction.

(b) With increasing specialization, surgery and anatomy as a prerequisite to surgery have diminishing claims on curricular time for the many medical graduates who will never be surgeons.

(c) The physiologists of an earlier vintage did their job so well that research and teaching of human physiology *sensu stricto* is no longer the terrain of any one department, and can be adaquately accommodated at one or other level by departments of pharmacology, experimental pathology, clinical medicine, brain surgery and neurology—each with whole-time staffs in the post-war set-up.

Since it is indisputable that the overwhelming majority of grammar schools are not, and in my view never will be, equal to the task of providing a pre-medical course in biology of a scope comparable to that of the Universities of Cape Town and Aberdeen during my own tenure of office, we may dismiss the possibility that any change

[1] The Antennaless Saga mentioned here and elsewhere was the joint contribution of Gordon and Sang, the latter continuing to make important contributions to the Nature-Nurture issue.

of the content of a grotesquely inadequate School Certificate syllabus will accomplish the outcome which I have outlined. On the other hand, there is no reason why departments of so-called human physiology and of human anatomy should not, severally or jointly, adjust their terms of reference to provide a remedy for the deplorably low standard of school instruction in elementary biology now accepted as adequate. To be sure, few of the present incumbents of chairs are both equipped and disposed to implement such a proposal; but any university is free to make the change without friction, if it seeks out from the ranks of experimental zoologists successors for replacements whenever retirement leaves vacant posts to fill. In the academic set-up, let me point out, on the eve of my retirement, one enduring fountain of cheerfulness. While there is death, there is hope.

Meanwhile it should be the special responsibility of departments of social medicine to acclimatize medical students adequately indoctrinated by a composite approach to what experiment can teach us both about developmental physiology and about the genetic architecture of the zygote *vis-à-vis* the interaction of nature and nurture in the peculiar social environment of a species which continually reshapes its own surroundings. Short courses of human genetics based on pedigree material cannot accomplish this objective. On the contrary, they confuse the essential difficulties of interpretation, if for no other reason because the human family is the medium for transmitting a physical regimen and a social tradition no less than an assemblage of genes.

5

THE ASSESSMENT OF REMEDIES[1]

THE words *statistics* and *controlled* are ubiquitous in the contemporary literature of medical research; but few of us reflect on their literal meaning. In its original connotation, statistics pertain to affairs of state, and in that sense we may properly speak of the outcome of a prophylactic trial as a statistical issue. A controlled experiment on the behaviour of an animal accomplishes the end in view when it discloses a one-to-one correspondence of stimulus and response in a specifiable range of bodily states of a specifiable range of genotypes reared in a specifiable range of conditions relevant to their development. It then discloses a recipe for the control of animal behaviour.

The specification of the range need not always be explicit. If one says that a cat responds to intravenous injections of such and such a quantity of adrenaline by a rise of blood pressure, one invites little misunderstanding. Laboratory experience has abundantly shown that normally healthy cats of all common breeds (and crossbreds from matings of all common breeds *inter se*) do behave in this way. Contrariwise, the statement that human beings respond to a cereal diet of maize by becoming victims of pellagra is on quite a different footing. Some members of a community so fed manifestly do not succumb; and we know as yet little about which do or do not. As it stands, any such assertion may embody a useful recipe in matters pertaining to the state of the nation, but it can contribute little to our understanding of how to control the behaviour of the individual, unless we carry our inquiries a further step. This further step is the burden of my plea for a new approach to the *assessment of remedies*. At the outset, I therefore put to the reader the following question: Is the end in view the state of the nation or the state of the individual

[1] Reprinted from *The Medical Press*, October 13, 1954, Vol. CCXXXII, No. 6023.

patient? What we can mean by the *design* of a *controlled* experiment, and where, if at all, the professional statistician rightly comes into the picture depends on the answer we give to it.

In 1915, Udny Yule and Major Greenwood published a memoir on the statistical theory of large-scale tests to assess the value of immunization procedures. Their methods, amended and greatly extended by R. A. Fisher, have become the pattern of what is now widely regarded as a properly conducted clinical trial. To do justice to its claims, we must first be clear about the level at which Yule and Greenwood take up the discussion. Briefly, we may state the issue as follows: Given two attack rates referable to groups of individuals previously subjected or not subjected to an immunization technique, one approaches the problem with the presumption that one will not be able to record precisely identical rates for a second sample or a third sample treated in the same way. Hence arises the question: How big must be the difference between attack rates for a single treated and a single untreated sample, if we are confidently to conclude that treatment does *in the long run* lower the risk of contracting the relevant disease?

Exponents of statistical theory in the Yule-Greenwood-Fisher tradition have laid down certain criteria of decision associated with the statistical concepts designated *significance* and *fiducial probability*. An opposing school, led by J. Neyman and Abraham Wald in America, supported by Egon Pearson in this country, have elaborated different procedures associated with the statistical concepts of *dual risk* and *confidence*. For many years, roughly over the period 1935-45, the bulk of their professional contemporaries (and they themselves) regarded the issues at stake as a matter of refinement within a framework of commonly accepted assumptions. After 1940, the search for a formula to resolve differences was beginning to disclose a wide cleavage about fundamentals. What is now open to discussion transcends any preference for one or other technique. The controversy, which has assumed the aspect of an international crisis, has sharpened. Indeed, our colleagues in America have at least implicitly raised the question: Can a consistently objective view of the proper terms of reference of the mathematical theory of probability furnish any rational basis for the class of judgments associated with significance test procedure and with the concept of fiducial probability?

In short, statistical theory is temporarily, at least, in the quicksands. Nothing less than a transvaluation of all values is in process. For my part, I have reluctantly come to the conclusion that the statistician will emerge in the end with a very much chastened view of what traditional methods can accomplish. If there proves to be any enduring basis for a stochastic calculus of judgments, we shall be able to define its proper terms of reference clearly only after we have cleared the site from an overgrowth of prescriptions which can certainly no longer claim the universal assent of professional mathematicians.

While the biologist or the sociologist could still hopefully regard the current controversy as a family quarrel about issues of trivial relevance to the practical man, the publication of an attractively written manual by Bradford Hill, himself a disciple of Greenwood, made the traditional pattern of significance test procedures as prescribed for the prophylactic trial available to a growing and receptive body of clinicians and medical research workers engaged in evaluating the therapeutic uses of the sulfa drugs and of the antibiotics. By an irony of fate, the significance test drill thus reached the pinnacle of its presumptive usefulness during the period in which its credentials had become more and more a topic of dispute among statisticians themselves. In this article I do not propose to traverse the disputable issues. That they are indeed disputable invites us to re-examine what we are attempting to accomplish, when we undertake to assess the efficacy of a remedy.

If we then concede every claim put forward for such devices as the Chi Square test and others of its kind till recently prescribed by most professional statisticians without misgivings, we may still entertain misgivings about how far the questions for which they claim to prescribe the method prerequisite to a correct answer tally with what the clinician and the biological research worker do want, or should most want, to know in the context of the clinical trial. In stating my own view about this, I approach the topic with the admitted preoccupations of my main professional lifework as an experimental biologist. In that capacity, I see the assessment of remedies as the disclosure of a specific *stimulus-response nexus*; and I mean by controlled experiment no less exacting an undertaking than as stated in my opening paragraph.

In what follows, I shall discuss procedures for evaluating curative

or palliative measures on the ambulant patient (an individual) as well as the patient (an individual) confined to bed, explicitly excluding prophylaxis as an affair of state; and I shall use the term 'clinical trial' to cover any attempt to evaluate the efficacy of vitamins or venisection, convalescent sera or surgical excision, faradization or electroconvulsive therapy, rehabilitation exercises or ultra-violet radiation, soporifics or stimulants, drugs or deep analysis, and whether the end in view is cure or merely mitigation of the patient's lot. Each of the foregoing we may speak of generically as a *specific stimulus*, and we may likewise designate the specific outcome as the *response*. This is our yardstick of efficacy. In all attempts to identify a particular stimulus as an appropriate antecedent to a specified response it is necessary to adopt one of two methods of *controlled comparison* with which the experimental biologist has long been familiar:

(a) enumeration and specification of the responses of comparable groups of individuals when treated and not treated;
(b) Enumeration and specification of the responses to treatment and to withdrawal of treatment in comparable bodily states to one and the same individual.

Of these two methods, which I shall refer to respectively as the *Group Trial* and the *Self-controlled Trial*, the physiologist will prefer the latter when practicable. If it leads to the discovery of an invariable specific response to a specific stimulus in a wide range of bodily states, the circumstance that another individual behaves in a different way is then a direct challenge to face the question: What characteristics of the individual contribute to the stimulus-response nexus? Contrariwise, and especially if indoctrinated with statistical dogmas, the investigator will be more readily content to relinquish inquiry if able to place on record a clear-cut average difference between the responses of groups, subject to different stimuli. The investigator who does indeed relinquish inquiry at this stage abandons the intellectual task of promoting the advancement of biological science for the moral satisfaction of performing a social service.

When the statistician speaks of the design of an experiment, he, or she, means the design of a trial to ensure that it provides the requisite data for a test which (if valid) will justify or discredit the assertion that a treatment guarantees an average level of benefit for

a representative group of patients; but if we approach the problem of the clinical trial as the disclosure of a stimulus-response nexus, it is *not* our ultimate goal to state that treatment A is or is not better than treatment B in this sense, though such a statement may well be a useful and unavoidable preliminary. We are in search of a one-to-one correspondence, and any assessment of remedies stated within the linguistic framework of averages prescribed by the requirements of the statistician is at best a provisional answer to the question which medical ethic and biological curiosity alike prompt us to explore in the context of the clinical trial. This will be clear enough if we pause to consider a fictitious situation.

Let us suppose that there is no recovery from a disease *D*, if untreated. Let us also suppose that a group trial conducted in conformity with the Yule-Greenwood pattern has led us to assess the recovery rate under treatment A as about twenty-five per cent, and under treatment B as about fifty per cent. Are we to content ourselves lazily with the recommendation to step up the recovery rate twenty-five per cent by substituting B for A? If so, our preoccupation with averages has blinded us to biological realities. If we are alert to the manifold interaction of nature and nurture, the outcome will invite us to ask: What peculiarities are common to individuals who respectively respond or fail to respond to one or other treatment? It is then thinkable that the following situation might arise:

(a) Persons with grey or brown eyes invariably respond to treatment B and fail to respond to treatment A;
(b) persons with blue eyes invariably respond to treatment A but not to treatment B;
(c) Blue-eyed individuals and individuals with grey or brown eyes occur in the population in the ratio 25: 50;
(d) Our clinical trial groups are representative in the sense that the ratio of blue-eyed persons to persons with grey or brown eyes is close to 25: 50.

On these assumptions the recovery rate would be 100 per cent if all D patients with blue eyes continued to receive treatment A and all D patients with grey or brown eyes henceforth received treatment B. Here, I neither blush to state a proposition of so compelling simplicity, nor claim any originality for doing so. In his lectures on experimental medicine, that great pioneer of physiology, Claude

Bernard, gave the answer of the authentic experimentalist to the challenge of Quetelet's *Essai de Physique Sociale* (1835) with no uncertain sound, when he declared:

'By destroying the biological character of phenomena, the use of *averages* in physiology and medicine usually gives only apparent accuracy to the results. . . . If, for instance, we observe the number of pulsations and the degree of blood pressure by means of the oscillations of a manometer throughout one day, and if we take the average of all our figures to get the true or average blood pressure and to learn the true or average number of pulsations, we shall simply have wrong numbers. In fact, the pulse decreases in number and intensity when we are fasting and increases during digestion or under different influences of movement and rest; all the biological characteristics of the phenomena disappear in the average. . . . If we collect a man's urine to analyse the average, we get an analysis of a urine which simply does not exist; for urine, when fasting, is different from urine during digestion. A startling instance of this kind was invented by a physiologist who took urine from a railroad station urinal where people of all nations passed, and who believed he could thus present an analysis of *average* European urine! Aside from physical and chemical, there are physiological averages, or what we might call average descriptions of phenomena, which are even more false. Let me assume that a physician collects a great many individual observations of a disease and that he makes an average description of symptoms observed in the individual cases; he will thus have a description that will never be matched in nature. So in physiology, we must never make average descriptions of experiments, because the true relations of phenomena disappear in the average. . . . I acknowledge my inability to understand why results taken from statistics are called *laws*. . . . Certain experiments by which they found that the anterior spinal roots are insensitive; other experimenters have published experiments by which they found that the same roots were sensitive. These cases seemed as comparable as possible; here was the same operation done by the same method on the same spinal roots. Should we therefore have counted the positive and negative cases and said: The law is that anterior roots are sensitive, for instance, 25 times out of a 100? Or should we have admitted, according to the theory called the law of large numbers, that in an immense number of experiments

we should find the roots equally often sensitive and insensitive? Such statistics would be ridiculous, for there is a reason for the roots being insensitive and another reason for their being sensitive; this reason had to be defined; I looked for it, and I found it; so that we can now say: The spinal roots are always sensitive in given conditions, and always insensitive in other equally definite conditions.'

Against this background of lucid exposition (1865), it is at first difficult to understand why it should now (1954) be necessary to challenge the claim of the statistician to prescribe the *design of experiments* in general and of the clinical trial in particular. In my view, such claims are acceptable only if we relinquish the standards of intellectual rectitude of an earlier generation. That any such challenge is still indeed salutory is to me intelligible only if we remind ourselves that: (a) the intrusion of statistical procedures into the domain of the prophylactic trial antedates the prescribed contemporary pattern of the therapeutic trial by a long period; (b) statistical quality control of pharmaceutical products dictated a new attitude to the assay of drugs during a decade signalized by spectacular chemotherapeutic discoveries.

Advances of endocrinology and of the study of nutrition in the twenties created a new class of problems for the pharmaceutical firms, whose concern is less to guarantee the maximum error of estimated activity assigned to the individual product than to ensure an overall reliability satisfactory to the consumer. Consequently, the chemist's assay criterion has made way for the accountant's, and a growing body of medically trained personnel now accepts without demur a routine of standardization prescribed by commercial considerations devoid of relevance to the proper concern of the research worker now all too often acclimated to the idiom of averages in the setting of the prophylactic trial.

In theory, an investigator more alert to the niceties of the nature-nurture issue than are most exponents of environmental medicine and (*a fortiori*) most proponents of eugenic measures might approach the problem of preventing disease with due regard to the genotype of the individual and to his, or her, case history; but the prophylactic trial had been the playground of the epidemiologist long before the nature-nurture dichotomy clarified in modern times. Its story starts when vaccination had lately come into Western Europe from

Turkey at the beginning of the nineteenth century. Two so eminent exponents of the mathematical theory of probability in its classical period as Euler and D. Bernoulli wrote at length on the topic. In what seems to be one of the first text-books of medical statistics, Jules Gavarret (1840) set out what is essentially the current pattern of the clinical trial against the background of this discussion and under the impetus of Quetelet's platonic construct, the normal man.

Gavarret's teaching gained few contemporary adherents, perhaps because of the remarkable advances of experimental physiology in the ensuing generation under the leadership of Muller, Helmholtz and Claude Bernard; but the intellectual climate for its reception was more favourable when proposals for compulsory immunization became the focus of a violent controversy at the latter end of the nineteenth century. In this dispute, the explicit *casus belli* was merely an issue of *social accountancy*. As such, it invited a decisive answer in the idiom of averages; and biologists were by no means a united front against conscientious objectors. Indeed, Alfred Russell Wallace entered the lists as a protagonist of the antivaccination forces; and since biologists could not agree among themselves, they could resolve their differences without washing dirty linen in public only by referring the case to the higher court of statistics as the proper tribunal to arbitrate on averages. By 1915, when Greenwood and Yule published their notable memoir on the statistical theory of the prophylactic trial, public health authorities were thus ready to accept the statistician's verdict as final.

Still, there were dissentient voices among the clinicians, notably and lucidly that of the vigorous and often wrong-headed Sir Almroth Wright, whose protest, if unexceptionable at a verbal level, gained no reinforcement from the standards of evidence contemporary clinicians condoned. Till the early thirties, the clinician was content with the all-too-familiar *follow-up* as a justification for preferring a new to an old treatment, or vice versa. Referable only to case histories of patients willing to testify or to report for examination, commonly with no control group for comparison, usually with no safeguard against highly biased selection of cases treated and rarely undertaken with precautions to validify the patient's testimony or the author's clinical judgement, a clinical trial so conceived violates any or all of the canons of scientific method, contravenes current statistical doctrine about group comparison, and contributes nothing to an

elucidation of the deeper issues which current prescriptions for group comparison cannot clarify.

When therapeutics took a decisive step forward with the discovery of quinine substitutes for malaria treatment, of the sulphonamides and of the antibiotics during the period 1930-45, the time was thus ripe for the statistician to assert his claims as the honest broker in a new business enterprise. Nor need we conclude that the influence of the statistician has been wholly without benefit to the progress of therapeutics, if we recall the gentle grafter of O. Henry, an enthusiast who adhered strictly to the principle of always giving the customer some benefit for purchase, thereby avoiding any embarrassments that he could not dispose of by means of a ten-dollar bill, a five-cent cigar and a thorough knowledge of railway time schedules or routes across state boundaries. Admittedly, the addition of sand to paraffin sold accordingly at twice market price does not of itself promote better illumination, but the salesman consistently informed the housewife that she must more regularly trim the wick and meticulously polish the glass funnel, thereby accommodating profit with principle at least at the consultant level. Let us then admit that the intervention of the statistician has been thus far beneficial to medicine. It has taught the clinician a clean wick drill; the need to choose a control group as a basis for group comparison. It has initiated the clinician into the polished chimney routine, selection for comparison of groups which are as nearly comparable as human ingenuity can guarantee.

Since the to-date indisputably useful contribution of the statistician to the assessment of remedies has thus been to impose a discipline which is entirely consistent with the common sense of a biologist without statistical training, we may address ourselves to the question: What problems will remain for the statistician, if we design the clinical trial in conformity with the more exacting standards of experimental physiology? Let us now, therefore, examine in detail the problems of design as they severally arise, first when we employ the self-controlled method of presenting different stimuli repeatedly to the same subject, and, second, when we employ the method of presenting different stimuli to comparable groups of subjects.

The Self-controlled Trial. The work of Oliver and Schafer (1894-5) on the vasoconstrictor action of extracts from the posterior

lobe of the hypophysis and from the adrenal medulla illustrates a pattern of inquiry long familiar to physiologists. The outline is as follows:

We prepare an extract from a particular tissue (T) suspected as the seat of production or storage of an active component. We also make extracts (E_1, E_2, E_3, etc.) of a wide range of other tissues by exactly the same method. We successively introduce in exactly the same way equal quantities of T and of E_1, E_2, etc., into the circulation with equally spaced intervals of recovery in an order which excludes masking an authentic or disclosing a spurious effect by reason either of delayed action or of an intrinsic rhythm. Finally, we record the appropriately defined response as + or −. We then say that we have established the seat of the putative action substance if some such record as follows is the outcome:

Treatment	E_1	T	E_2	T	E_1	T	E_3	E_4	T	T	E_5	T	E_2
Response	−	+	−	+	−	+	−	−	+	+	−	+	−
Treatment	E_1	E_6	T	E_5	T	T	T	E_3	T	E_6	T	T	E_4
Response	−	−	+	−	+	+	+	−	+	−	+	+	−

One-to-one correspondence of stimulus and response is here the criterion of a conclusive outcome, and statistical assessment in the current sense of the term is not relevant to its interpretation. In the domain of experiment on animals the major desiderata of correct interpretation are therefore three:

(a) Reliable method of presenting the stimulus, e.g. in this context washing out the last remnants from the syringe into the circulation by immediate injection of an equal quantity of saline after the injection of each extract;
(b) a reliable method of recording the response, e.g. a kymograph in this context;
(c) A judicious variation of intervening intervals and sequence of presentation of putative stimuli to forestall spurious *s-r* correspondence attributable to delayed effects or to some periodic rhythm of response in phase with the application of a particular stimulus.

The third of these desiderata calls for no special comment in the context of the clinical trial, except to say that there seems to be no

intelligible reason to adopt the device of attaching random numbers to the stimuli (e.g. capsules), as we should deem to be desirable in the proper domain of stochastic inference. On the contrary, intentional disorder, i.e. a pattern assigned to make easier the recognition of delayed action and to forestall misinterpretation of a normal rhythm would appear to be a more reliable way of achieving the end in view.

The presentation of the stimuli to a human subject calls for special precautions to forestall responses evoked by concomitant stimuli at a cognitive level. When the aim is to assess the efficacy of a drug which takes the place of an active extract (T) in the foregoing example, an inert *placebo* (E_1) and it may well be a second drug (E_2) of known activity will take the place of the non-active tissue extracts. The subject should receive each stimulus in a capsule of standard size, texture and colour to prevent recognition; but it will be wise to eliminate possible clues arising from the manner of presentation. The procedure recommended by Hogben and Sim (1953)[1] is as follows:

(i) the *physician-investigator* supervises the allocation of code numbers or letters to the stimuli for presentation in the pre-assigned order;
(ii) an *intermediary* (e.g. nurse, orderly, houseman) equipped with the code signs and the programme of presentation has no knowledge of the code;
(iii) the *subject* who receives the stimulus from the intermediary does so without access to the code sign.

The indispensable desideratum of a reliable record when the subject is human is a daily *proforma*:

(a) explicitly specifying *all* information required to be supplied by the subject and/or field worker;
(b) explicitly indicating the time of day at which each item of information is required to be completed;
(c) set out in a form to ensure minimum effort of subsequent analysis by the investigator;
(d) expressed in terms carefully chosen to forestall any error of interpretation with due regard to the intelligence and educational level of the subject;

[1] *Brit. J. prev. soc. Med.*, 7.

(e) providing both internal and external means of checking the intrinsic value of the information recorded.

The use of some drug (E_2) of known activity in addition to the placebo (E_1) as a control is one example of what I here mean by an external check; but the need to distinguish between internal and external checks on verbal testimony arises from two different levels at which they may be useful, and emphasizes the need for a more refined distinction than the traditional dichotomy of signs and symptoms. If the subject's testimony relates to events which any other person would describe in the same terms, the record of his or her own verbal responses is directly assessable by external checks, and the mere fact that it is a statement about himself or herself (e.g. frequency of micturition) does not set it in a category apart from other observations. Another class of assertions relative to the *self* are not self-made *observations* in the commonly accepted sense of the term, i.e. in so far as the meaningful content is manifest at its face value. None the less, the investigation of their consistent relation to the subject's past experience, bodily states, current environment in general or treatment in particular, may lead to an interpretation which confers on them a hitherto unsuspectedly meaningful content, as the study of referred pain testifies.

For lack of a current term, we may speak of a verbal response of the second sort as *non-observational.* Though the interpretation may have little or no connection with the *manifest form of words*, any regular association between external occurrences of observable bodily states and a verbal response at the non-observational level so defined may well be an intrinsically valuable addition to our knowledge of human behaviour in its own right. Regardless of its ostensible bearing on the end in view, it is therefore desirable that the record should incorporate as much observational testimony amenable to direct observation, and hence to external checking, as is consistent with limitations imposed by the educational level of the subject and his or her appetite for co-operation. Inclusion of such ancillary information in the daily record is also desirable, but not always for another reason. In some inquiries it may suffice for the end in view, if the identification of the treatment procedure does not emerge at the level of cognition. In others it may also be essential to keep the subject in ignorance of the intention, and it will be less easy to

accomplish this, if the items of the *proforma* are exclusively relevant thereto.

Among data for the background picture of the subject's discharge of normal bodily functions and assessment of the subject's reliability as a clerical assistant in the investigation, times of defecation and micturition, body weight, times of retiring and rising, hours of work and physical exertions, are appropriate items of the self-record for subjects of any educational level. After brief preliminary tuition, there is no reason why intelligent volunteers with a good high school record should not also undertake to complete at suitably prescribed intervals entries for pulse and respiration rates, body temperature and blood pressure.

The inclusion of ancillary information in the daily record, i.e. of information which is not directly relevant to the specification of the response, is essential for a reason other than as a means of checking reliability of testimony. Since the immediate end in view is to establish a one-to-one correspondence of stimulus and response on the part of the individual in a *specifiable range of bodily states*, any information bearing on the variability of the internal environment may turn out to be relevant to the interpretation of the record. Nor does the design of a daily record to accommodate such information complete our task, which must take within its scope individual variability. It will therefore be necessary to provide for one or more predesigned and precoded documents recording:

(a) physique;
(b) functional state assessed by biochemical and biophysical tests;
(c) personality structure.

The only important limitation to the use of the self-controlled trial in experimentation on animals with a view to disclosing a stimulus-response nexus is that the response is repeatable within a comparatively short period of time. This, of course, excludes the study of irreversible responses such as growth. In the domain of human remedy assessment we have admittedly to contend with other limitations which ethical considerations dictate; but I cannot agree with a writer in the *British Medical Journal* (June 12, 1954) that 'the solution proposed would work best when least needed—that is, when knowledge is most complete. It would offer little guidance to

an investigator struggling, as most investigators are, with interesting but obstinately variable phenomena.'

In this pronouncement the writer seems to be unduly preoccupied with the killing diseases that medicine has conquered or may soon conquer. In Western communities where the expectation of life at birth is well over sixty years, a medley of minor and recurrent maladies, including what we vaguely call the psychosomatic disorders, rank high among the priorities of medical research. For such, the method of self-controlled comparison offers opportunities for study where group comparison is impracticable. To the extent that it is possible to design records which conform to the requirements stated, it will be possible to undertake a comprehensive programme of research in hitherto neglected fields of low-grade morbidity by enlisting volunteer subjects in a *normal environment* without recourse to expenditure on living accommodation.

The Group Trial is, as stated, an indispensable tool of physiological research in so far as many responses are irreversible. It is also more convenient than the alternative when an interval exceeding an hour or more must intervene between presentation of successive stimuli. In the self-controlled procedure, the main concern of the investigator is with variability of body states which may mask the *s-r* relationship or disclose one which is spurious. In group comparison individual variation is the major source of disappointment or error. The physiologist deals with it in two ways. If he has at his disposal animals which breed in the laboratory he is free to use highly inbred stocks kept under standard conditions of diet, humidity, temperature, illumination and accommodation. If he is dealing with captive animals of unknown parentage and nurture, he will pair off animals of like sex, weight and otherwise recognizable similarities. In either situation, the physiologist's aim is to be able to assert with assurance what animals of a specifiable stock, age, body weight and so forth under standard conditions of diet, temperature, etc., invariably respond in a particular way.

Needless to say, this is not the statistician's aim in a clinical trial based on group comparison. We start with the difficulty that we cannot use genetically standardized stocks maintained in a standardized environment from birth; and if we pair off patients as much alike as possible we may be vitiating the statistician's demand for completely random choice implicit in the theory of a test or interval

estimation procedure. In any event the statistician's predilection for groups which are representative in terms of random choice may well discourage the initial exercise of clinical judgement w.r.t. selection of subjects for comparison. If a disease is invariably fatal without treatment, the physiologist faced with the fact that some subjects, if treated, survive, will interpret the disclosure as an invitation to ask: What characteristics of the individual determine his response to treatment? The statistical design of the experiment of itself discloses no clue to the answer.

More usually the situation is less simple. If we compare two treatments A and B, it may happen that a proportion x_a and x_b in each group respond *positively* (benefit), y_a and y_b fail to respond without deterioration, while z_a and z_b respond *negatively* (deteriorate). In each treatment group we may thus presumptively have individuals whose potential response to the alternative treatment is classifiable as $+, 0, -$. To identify them as such is to the physiologist the true aim of the trial; but traditional statistical procedures take no stock of this possibility which Wrighton (1953)[1] in a brilliant and challenging recent publication does indeed discuss as a statistical issue *per se*. In so far as the method of group comparison can conform to the recognized standards of experiment on animals, it is therefore necessary to incorporate in our observation record far more information than statistical theory encourages us to collect. No less for the group trial than for the self-controlled trial, it is essential to have on record for each individual every item of case (including family) history conceivably relevant in the light of clinical experience to his or her response to treatment.

The discussion of either method of comparison we may adopt for the assessment of remedies thus forces on our attention one useful service that a department of statistics can discharge in a medical school. The clinician concerned with the assessment of remedies has rarely had prolonged and intimate experience of laboratory work on animals. As a medical clerk or houseman he or she has acquired a habit of narrative description doubtless contributory to the success of many of his kind in the realm of drama and fiction, but little likely to inculcate the art of planned observation requisite to biological design of what is (after all) experiment on human subjects. Since the primary concern of the good physician is for the welfare

[1] *Acta genet. statist. Med.*, 4.

of the patient himself or herself, there must needs be an enormous wastage of valuable information relevant to the requirements of research, if the clinician relies on personal inclinations to determine what is or is not worth recording.

It may be true, and I think it is true, that the theory of probability has little to do with the design of records for research on human beings; but the design of records (as I have elsewhere discussed[1] at length) is an expert task for which the medical student has hitherto received no training, and one which no other department of a medical school is likely to undertake if its statistical department does not provide such training. Unhappily, senior clinicians and Ministry officials still regard the design of medical records as an assignment for the clerical staff. The truth is that right design calls for mature experience of research, wide knowledge and the exercise of a high level of logical sophistication. It is my belief that general, no less than consultant, practice might participate more actively in the advancement of medical knowledge, if medically qualified personnel were more willing to seek expert advice on the design of records.

[1] Hogben L. (1951): *Incorporated Statistician*, 2; Hogben L., and Cross (1954): *Brit. J. prev. soc. Med.*, 8.

6

THE PRESENT CRISIS IN STATISTICAL THEORY

IT is not without reason that the professional philosopher and the plain man can now make common cause in a suspicious attitude towards statistics. We witness on every side a feverish concern of biologists, sociologists and civil servants to exploit the newest and most sophisticated statistical devices with little concern for their mathematical credentials or for the formal assumptions inherent therein. This state of affairs would be more alarming as indicative of the capitulation of the scientific spirit to the authoritarian temper of our time, if it were easy to assemble in one room three theoretical statisticians who agree about the fundamentals of their speciality at the most elementary level.

No one who reads reflectively the discussion on a recent contribution of Anscombe in the Journal of the Royal Statistical Society (Series A, vol CXI, 1948) will regard the last sentence as overstatement. After a generation of prodigious proliferation of statistical techniques whose derivation is a closed book to an ever-expanding company of avid consumers without access to any sufficiently simple exposition of their implications to the producer-mathematician, the challenge of J. Neyman, E. S. Pearson and Abraham Wald is provoking, in Nietzsche's phrase, a transvaluation of all values. Indeed, it is not too much to say that it threatens the entire superstructure of statistical estimation and test procedure erected by R. A. Fisher and his disciples on the foundations laid by Karl Pearson, Edgeworth and Udny Yule.

The word statistics has at least six different meanings in current

[1] An Address delivered in University College Aberystwyth and reprinted from *The Incorporated Statistician*, May 1956.

use, four in the context of statistical theory alone. Since unduly pretentious claims put forward for statistics as a discipline may well derive a spurious cogency from the protean implications of the word itself, let us here take a look at the several meanings it enjoys in current usage. We speak of numerical data as statistics, e.g. of population or of exports; and we may so speak of arithmetical devices for summarizing them, as when we use the terms vital statistics or economic statistics. However, such procedures as the construction of life tables or of a cost of living index rely on arithmetic and common sense only. The principles invoked do not come within the province of what we commonly call the theory of statistics today. All procedures subsumed by theoretical statistics as we now use the term invoke the calculus of probability; and it is by no means obvious that they do so with equal justification. A fitting background for a fruitful re-examination of their several aims is therefore the highly controversial issue: in what situations is a formal algebraic theory of probability relevant to human experience?

Our task would be less exacting if all mathematicians could agree about a formal definition of probability. This is not so. A brief review of the history of the problem will show that it is now less so than ever before. To trace the history of the calculus of probability we need go no further back than a correspondence between Pascal and Fermat early in the seventeenth century. The occasion was the plight of Chevalier de Méré, a great gambler and by that token *très bon esprit* but alas (wrote Pascal) *il n'est pas géomètre.* Alas indeed. The gallant gamester had made a pile by always betting small favourable odds on getting at least one six in four tosses of a die. Then he lost it all by always betting small favourable odds on getting at least one double six in twenty-four double tosses.

In Pascal's treatment of the problem we assign to each possible outcome of a single trial an equal opportunity to associate with every possible outcome at a second, at a third one and so on in the composite r-fold trial. We may properly speak of the ratio of the number of different r-fold sequences scored as a success in accordance with the prescription of the game and of the betting rule to the total number of different r-fold sequences as the *proportionate possibility* of success. Pascal's followers have called it the *probability* of success; but there is indeed no obvious connection between the term so used

and what we commonly mean by probability in the realm of judgements about future occurrences. To establish any such connection at a factual level, we must first concede a proposition they themselves assumed to be self-evident. If the instructions for manipulating the toy and its make-up are such as to safeguard *equipartition of opportunity to associate as the eventual outcome of a sufficiently long series of trials*, probability so defined subsumes the long-run frequency of success.

In its own context, this intuitive assumption doubtless incorporated a wealth of authentic factual, albeit not as yet systematically recorded, experience about what happens in the gaming saloon. Since Pascal wrote on the subject, large-scale trials of various sorts have abundantly shown that probabilities calculated accordingly in the light of plausible assumptions about the behaviour of dice, lotteries, well-shuffled card packs and other devices used in so-called games of chance tally with observed frequencies as closely as the intuitive faith of the Founding Fathers invites us to believe. To that extent, the gambler uncertain about the outcome of any single trial, may feel confident of solvency and/or profit in a sufficiently protracted series of games, if he sets his stakes in accordance with the classical theory of wagers. However, two important qualifications circumscribe this tie-up between any formal definition of algebraic probability and what happens in the real world in the long run or between our definitions and how we use the word probability more or less meaningfully in the domain of judgment.

Of these, one involves the prescription of the game, the other the deportment of the player. First notice that we are dealing with a threefold situation involving an agent, an apparatus and a task which transcends the limits of the sensory discrimination of the agent within the framework of a programme of instructions which we now speak of as a randomizing procedure, e.g. shuffling or spinning in a particular way. Next notice that the theory guarantees success only if we state in its *entirety and in advance* a rule to which the gambler must adhere consistently *regardless of the fortunes of the game*. In short, we have as yet gained no obvious clue to the recognition of situations of which we can predicate randomwise behaviour outside the domain of games of chance; and we have as yet no title to associate the application of algebraic probability with judgments about occurrences in the past. However, we are now

ready to formulate more explicitly what sort of betting rule the classical tradition endorses.

At the outset, we must recognize that we can make no such rule without prior knowledge of the definitive parameter or parameters of the stochastic system, e.g. $p = \frac{1}{2}$ for the toss of a coin or $p = \frac{1}{6}$ for the toss of a die. Admittedly, we cannot make very precise statements of this sort on the basis of what we know about the construction of the toy alone; but an appropriate system of asymptotic scoring, as lately prescribed by Wrighton, will justify an appropriate assertion about the behaviour of our toy in an indefinitely protracted sequence of trials. *Ipso facto* such an assertion has no relevance to the outcome of a single trial. It can justify itself only by perpetual repetition in a sequence of comparable trials. So our betting rule will avail us nothing unless we follow it consistently. On that assumption, and only on that assumption, we can assign a long run frequency of error to the assertion embodied in the rule, a simple example of which is the following: *say that the result will be a score of at least one head whenever you toss a penny ten times.* For $p = \frac{1}{2}$, the long run frequency of the event last stated is 1,023 in 1,024 trials, so that the long run frequency of erroneous statement is less than 1 in a 1,000. On this understanding, we may say that $P_f < 10^{-3}$ is the *uncertainty safeguard* of the rule: always say the head score $x \geqslant 1$ when you toss a penny ten times.

This uncertainty safeguard is a property of the *endless* application of the rule. As we have seen, it has no relevance to the verdict we pass on a single application. We have also seen that the correct specification of a definitive parameter such as $\frac{1}{2}$ for a penny or $\frac{1}{6}$ for a cubical die implies a programme of action other than persistent assertion. We are here speaking of the parameter of a stochastic system, i.e. a situation in which the operation of a randomizing procedure guarantees a sequence of events consonant with what I have elsewhere called equipartition of associative opportunity. Hence our formulation of the pattern of stochastic reasoning excludes situations in which we cannot legitimately assume the existence of a randomizing process and includes only situations which fall within a range of historic framework of potential repetition.

Accordingly, a re-orientation of interest towards the end of the classical period which extends from Pascal and Fermat to Euler and d'Alembert signalizes two innovations to which the classical theory

gives no sanction. The occasion was the rise of Life Insurance at the end of the eighteenth century. Superficially, and especially in the same milieu, there was a close similarity between the aims of the promoters of insurance corporations and those of promoters of the Royal lotteries which accommodated the declining fortunes of the French monarchs to the rapacity of their mistresses and to popular resentment against the salt tax. Still, the intentions of the promoters should not blind us to a two-fold distinction. The lottery is a stochastic system in the classical sense and its promoters operated it in accordance with the Forward Look, that is to say, they stated a rule in advance and stuck to it. There is no *prima facie* case for the contention that life insurance is like a lottery in either sense.

In what (if any) sense we can rightly speak of life and death as a game of chance is a highly debatable issue. Even if we concede the propriety of relinquishing to a Divine Dealer the responsibility for operating a truly randomizing procedure in the act of shuffling the pack of Doom, any relevance of the classical theory to mortality experience presumes both the unchanging constitution of the pack itself and an unswerving adherence of the player to a betting rule stated in advance. In the context of Laplace, it was perhaps pardonable to dismiss the first of these postulates, because people were not as yet acclimatized to the changing card pack of a declining death rate; but the practice of the first successful life insurance agencies, notably the Equitable, was at no time consistent with adherence to a rule stated in advance and enforced regardless of the fortunes of the investors.

Dr Price, the first actuary of the Equitable, was indeed the first writer on probability to make a decisive breakaway from the classical tradition in notes he appended to a paper communicated in 1763 to the Royal Society after the death of its author. The author himself was Thomas Bayes, like Price a Unitarian minister in the Priestley circle. There is still room for argument about the intentions of a theorem stated in his posthumous memoir, and more especially with respect to a *scholium* posthumously found among the author's notes. Whatever his intentions may have been, the commentary of Price advances a novel idea which Laplace elaborated as the doctrine subsequently called inverse probability, i.e. that a calculus of probability entitles us to assign a numerical value to the credibility of our judgments about antecedent events on the basis of present and past observations.

The innovation for which Laplace was mainly responsible invokes the so-called *principle of insufficient reason* adumbrated in the celebrated posthumous *scholium*. Briefly, this signifies that we may assign to conceptually alternative events, either of which may have happened or will happen, a probability numerically equal to one half, when we have no prior knowledge about which has indeed happened or about which will indeed happen. In the same idiom, probabilities of unity and zero respectively denote the certainty that: (a) one alternative has happened or will happen; (b) one alternative has not happened or will not happen. These postulates suffice to define probability for the elaboration of a calculus of which the rules are exactly the same as those to which the classical formulation leads; but such formal identity of the outcome conceals a total difference of intention. Defined in terms of insufficient reason, probability has no reference to the frequency of observable events in the external world. There is therefore no common court of appeal for the sceptic and for the faithful.

If we are willing to take the *scholium* on trust, the use of the calculus to assess the value of retrospective judgements will evoke no intellectual discomfort; but the principle of insufficient reason has at no time engaged the universal, or indeed widespread, assent of mathematicians since Boole pointed out that there is insufficient reason for subscribing to it. Today, we may distinguish three schools by the attitude each adopts towards it. Jeffreys accepts the doctrine of Laplace in all its implications, including a redefinition of algebraic probability in terms of knowledge and ignorance. A second school led by Neyman and the late Abraham Wald in America adheres to the Forward View of the Founding Fathers. Between these extremes R. A. Fisher adopts an eclectic and puzzling position. He rejects the doctrine of inverse probability in so far as it relies on the Scholium of Bayes, but he reserves the right to look backward.

The calculus of permutations subsumes all the rules of the classical theory of wagers; and a modern restatement of the initial definitions in terms of the Theory of Sets raises no new issues relevant to its legitimate applications. If we adopt the principle of insufficient reason, we explicitly renounce the obligation to relate the mathematical definition of probability to the frequency of external occurrences; but any other way of defining algebraic probability

without explicitly invoking the notion of frequency relinquishes to those who apply it in practical affairs all responsibility for justifying the factual relevance of the definition itself. Thus many mathematicians who decline to take the principle of insufficient reason on trust have felt the need for equipping the algebraic concept of probability with some more explicit indications of its usefulness.

Matters came to a head in the middle of the last century, when Venn and others identified the probability of A, the possible outcome of a trial of which the result may be A or B, with the limiting value of the proportion of N trials in which A occurs as N becomes indefinitely large. A frequency definition conceived in these terms had a remarkably long vogue before its proponents recognized a glaring *non sequitur* to which it had committed them. As it stands, its ostensible factual form embodies no guarantee of the relevance of the rules of the calculus to what Venn called the series of the event. To endorse them, we need to specify something more. The series of the event must have the property I have named equipartition of associative opportunity. A shorter name for it is *disorder*. It was the contribution of von Mises to formalize this addition in the first quarter of our own century.

From the standpoint of the mathematician, von Mises accomplished the task of incorporating both the factual notion of frequency and the factual property of disorder more commonly called randomness in a single definition which suffices to endorse all the accepted rules of the calculus. This formal exploit does not wholly resolve an outstanding dilemma. It is not the province of mathematics to disclose the factual circumstances in which randomness as von Mises defines and refines the concept of probability arises. By the same token, it is not the prerogative of the mathematician as such to define what fields of practical endeavour the calculus of probability can help us to cultivate with profit except in so far as the new frequency theory proscribes recourse to the doctrine of inverse probability. If reliance on the calculus of probability is all that is common to what we now call theoretical statistics, it is therefore becoming to scrutinize with great circumspection the several classes of practical problems it claims as its province.

Needless to say, games of chance call for no further comment. On this understanding, it will be convenient to make a fourfold

division of the present subject matter of statistical theory; and it will help us to get controversial issues sharply into focus, if we trace separately the history of each such division. I shall refer to them respectively as the *Calculus of Aggregates*, the *Calculus of Error*, the *Calculus of Exploration* and the *Calculus of Judgments*.

By the *Calculus of Aggregates*, I mean the applications of the calculus of probability in the experimental sciences to the construction of hypotheses which invoke stochastic models, i.e. models which presume randomwise behaviour of subsensory particles on all fours with what we likewise presume about our urn models in the classical theory. This subsumes the entire field of physical hypotheses called statistical mechanics, including the pioneer formulation of the Kinetic Theory of Gases by Clausius and Maxwell and the more modern developments of quantum theory. It also embraces the genetical theory based on the experimental work initiated by Maxwell's contemporary, Mendel. We may here dismiss all such hypotheses briefly and honourably. The usefulness of any physical model depends on whether it leads us to hitherto unexpected but verifiable conclusions, and stochastic models are not different in this respect. Like others, they have to live dangerously; and we judge them by their fruits. If no one is in doubt about the meaning of the statement that the calculus of probability *works* in this context, let us therefore be all the more on guard against extrapolating our confidence in its usefulness. Confidence in its limited usefulness in the domain of experimental science has no bearing whatsoever on its claims to usefulness in situations which offer no unique opportunities for experimental verification of its relevance.

The proper domain of a stochastic *Calculus of Error* is what we associate especially with the names of Gauss and Hagen at a time when canal and railroad surveying created a new demand for optical precision and glass of better quality made possible new advances of astronomy, in particular the detection of the annual parallax of a fixed star. We sometimes speak of it as the theory of the *Combination of Observations*. At a factual level, we may plausibly regard the *accidental* errors of the observer as phenomena on all fours with score components of a player in a game of chance, and to that extent its initial assumptions rightly come within the scope of the calculus of probability. To apply the latter, we need, however, to invoke certain assumptions which are more questionable. The player must win in

the long run in the sense that the mean of an indefinitely protracted sequence of trials is the sought-for true value of a metric or physical constant.

It is pertinent to emphasize this initial assumption because we so often hear nowadays that scientific law is essentially statistical, whence the notion of absolute truth is *démodé*. So far as the progress of physics bears on the assertion, this prompts two comments. The first part of the statement unjustifiably identifies law exclusively with hypotheses which invoke stochastic models. The second excludes the only other use the physicist makes of the calculus of probability. The assumption of a *true* value which we seek as our intelligible goal in the theory of the combination of observations is the only firm foothold we have for a definition of what we mean by an accidental *error*.

In practice, we have a limited series of observations on which to rely; and the most we can legitimately hope to achieve is to state a rule of stochastic reasoning in terms consistent with the classical theory. We may set the uncertainty safeguard (P_f) of our wager at an *acceptable* small limit $P_f \leqslant \epsilon$, if our rule asserts that a true value (M) lies within acceptably small *preassigned* limits $(M_0 - x) < M \leqslant (M_0 + x)$ for an observed mean M_0. In the original statement of the theory, one definitive parameter of the stochastic system is exactly determinate only if we indeed know the true value M, whence a wholly satisfactory treatment of the problem is unattainable. In contemporary terms, we may admittedly remove this restriction by invoking the so-called t-distribution, uniquely determined by the spread of our observations; but we cannot then prescribe *beforehand* the size of sample which will guarantee a preassigned uncertainty safeguard for preassigned limits which are themselves acceptably small. Indeed, we cannot guarantee that such limits themselves will be acceptably small for any finite sample.[1] Accordingly, we relinquish the possibility of stating in its entirety any useful rule in advance. There is indeed as yet no rigorous stochastic regimen

[1] Neyman refers to this so recently recognized dilemma in the concluding remarks of his paper (1949) to the XVIII *Congr. Intern. Phil. Sci.* on the *Foundations of the General Theory of Estimation*. As he himself handsomely concedes in *Lectures and Conferences on Mathematical Statistics and Probability*, p. 256 (2nd Edition), Washington, 1952, it was J. Berkson who brought the issue to a head by pointing out that an infinite interval is consistent with what the t-distribution can accomplish for a preassigned uncertainty safeguard.

for the combination of observations consistent with the Forward Look.

The kingpin of the traditional theory is the frequency distribution of the mean error. This distribution tallies as closely as need be with our experience of certain types of repeated observations. Its most conspicuous features correspond in a rough and ready way with the frequency distribution of many natural phenomena such as heights of men of the same age or ten-year averages of rainfall in a particular locality. Seemingly, the first person to notice this was Quetelet, tutor to Albert (later the Prince Consort), Belgian Astronomer Royal and a professor of Geodesics in the Belgian Military Academy during the decade when the Gaussian theory of Error was taking shape. Captivated with a coincidence, Quetelet abandoned astronomy, surveying and meteorology to found a new science of *Physique Sociale* in the belief that numerical industry can establish eternal periodicities of social events on all fours with Kepler's laws of planetary motion.

In Britain, where Quetelet enjoyed the patronage of Albert, himself president of the session of the British Association made memorable by the announcement of Maxwell's theory, Galton enthusiastically embraced his views as a recipe for elucidating the laws of inheritance. His disciple Pearson followed suit, and adapted the entire formal algebra of the Gaussian calculus of error to the pastime of fitting curves to biological and sociological data. Such is the scope of what I have called the stochastic *Calculus of Exploration.* It embraces the twin techniques of so-called *Regression* of Multivariate Analysis and of *Factor Analysis.* The exploits of draughtsmanship the Quetelet *mystique* encouraged held up the study of genetics for a generation. After that the psychologists took it up. The psychologists themselves are beginning to have doubts. It now has a firm foothold in the social sciences. Otherwise, we might dismiss it as an anachronism.

Its credentials call for examination at more than one level. At one, it is exceptionable because the undertaking violates two canons of stochastic reasoning already discussed in the context of emergent life insurance. It relegates to a Divine Dealer disguised as Nature the responsibility for operating a shuffling procedure, unless we can advance satisfactory reasons for believing that the classical calculus of chance has a special relevance to such phenomena as age of death

of mothers of a specified income group. It also deals largely with phenomena which are unique historical events, excluded as such from any putative framework of repetition to confer on such a calculus the opportunity of vindicating its relevance.

The force of both these objections will be clear at the most elementary level, if we contrast: (a) what we do when we fit a straight line to the stretch of a spring of given material and dimensions for fixed loads on the basis of successive observations of the length recorded at one and the same tension; (b) what (if anything) we accomplish by plotting maternal age at death against family income. Our observations on the spring lead us to assign a linear constant which we can tabulate for subsequent use within the same range of loads for any spring of the same material and dimensions, and our assumption that the occurrence of accidental deviations from the true-long-run mean of stretch for any fixed load is at least consistent with our three-fold formula (p. 96) for a randomizing procedure. Nothing we can do about the age of mothers at death has any relevance to this formula and the so-called regression coefficient which defines the slope of the so-called best-fitting line for our graph has no operational value comparable with that of the elastic coefficient of a spring. We cannot confidently tabulate it as definitive of a social situation we are likely to encounter subsequently. Only the common algebra we invoke sustains the illusion that the situations themselves are comparable.

If we scrutinize the statistician's line of best fit more closely, the relevance of the Gaussian algebra either as a method of *point-estimation* or as a method of defining limits within which the value of a physical constant lies becomes even more debatable. The points the line cuts in the record of the physical experiment define where we believe our observations should lie, and where they would indeed lie if we could completely control the vagaries of our recording instruments. Thus the long-run mean of our successive observations of one variable for a single value of the other signifies the true value prescribed by a law which transcends human error. Contrariwise, the regression graph records a scatter of points which remain scattered in the absence of any defect of observation. The line that goes through the mean of any number of such points referable to a fixed value of one variable does not necessarily cut the grid at a point which records any existent value of the alternative variable.

When the physicist applies the theory of combination of observations to the determination of parameters which will later appear in a handbook of tables of physical constants, the outcome is useful as such because a so-called independent variable of a physical law is under the control of the operator. Such control is never certainly a realizable outcome of a social situation in which we apply regression analysis. We are then passive spectators. By increasing the number of contributory variables which multiple regression may identify as such, we do not necessarily arrive at any certain recipe for intervention. All we can hope to accomplish is a fuller statement of social accountancy. Conceivably, a descriptive exploit of this sort may have a limited usefulness in business or administration; but it scarcely justifies its title to a place in the enduring corpus of scientific knowledge.

If we entertain the hope that the algebra of Gauss can lead to statements worthy to rank as scientific law in situations which offer no opportunities of active intervention, we have indeed left behind us the world of physical reality to travel in the shadowy domain of Plato's universals. This much was clear to many of Quetelet's contemporaries, notably the French mathematician Bertrand, who stated with greater vigour and with enviable wit the case I have outlined. Since then, there has been a luxuriant overgrowth of algebra for which the exponents of Quetelet's principles have advanced much wider claims; but no one has disposed of Bertrand's critique to my satisfaction. Perhaps this is because the issues at stake, though concealed by an impressive façade of symbols, are not really mathematical. We are still waiting for an acceptable answer to the question: what is the charity in aid of? Pearson sought to dispose of it by advancing a novel redefinition of scientific law; but few people now feel confident that his *Grammar of Science* greatly illuminated the search for nature's laws or the definition of what we mean by one.

Oddly enough, no contemporary has responded to Bertrand's unanswered challenge. The focus of current controversy concerning the proper use of a calculus of probability in the biological and social sciences has been two techniques respectively called *test procedure* and *interval estimation*. These circumscribe the subject matter of what I here mean by the stochastic *Calculus of Judgements*. If we interpret the expression literally, we may nominate Laplace as

the parent of the programme; but the means now adopted scarcely took shape until medicine invoked statistics as the court of appeal in the controversy over compulsory vaccination. Indeed, the ostensible aims were not clearly definable till a keen debate beginning in the mid thirties forced the contestants to clarify their assumptions.

We may briefly define a test procedure as a rule for accepting or rejecting one or more hypotheses with a probability of error not exceeding some assignable upper limit. The qualification last stated signifies what I have elsewhere called an *acceptable uncertainty safeguard.* On one view, that of R. A. Fisher and his followers, the uncertainty safeguard is a property of the verdict passed on a single sample, and the undertaking carries with it no obligation to state the size of the sample in advance. In my view, which endorses that of Neyman, of E. S. Pearson and of Wald, this is inconsistent with a behaviourist evaluation of the proper uses of the calculus of probability. It is intelligible only if we accept what Fisher himself declines to do, that is to say the doctrine of inverse probability.

Divergent views about the prescription of a test procedure advocated by the two schools do not necessarily reflect this difference, since we can express either prescription in terms consistent with the obligation to state the size of the sample in advance on the understanding that the uncertainty safeguard is merely a property of a rule applied consistently within a framework of endless repetition. If interpreted in that way, we may speak of Fisher's prescription as a *partial*, in contradistinction to a *comprehensive*, decision test.

A stochastic test procedure of either sort presupposes:

(a) sample of occurrences reducible to *scores*, e.g. number of heads in five tosses of a coin;

(b) an assignable long run *frequency distribution* for the event, i.e. score or range of score values, e.g. five tosses will yield a score of at least four heads in three sixteenths of all samples of an unending five-toss sequence;

(c) one or more *test hypotheses* which specify some numerical constant of the frequency distribution, e.g. that the frequency of heads in the unending sequence of coin tosses is $p = \frac{1}{2}$;

(d) a score *rejection criterion* which defines a rule of decision involving an instruction to reject such and such a hypothesis A

whenever the sample score exceeds or falls short of a certain value;

(e) an *uncertainty safeguard* which specifies an upper limit to the long run frequency of erroneous statement, i.e. of rejecting a hypothesis when it is right.

A *comprehensive* test procedure invokes alternative test hypotheses H_0 and H_1 within the operation of a single rule of the following general pattern for a score s and a rejection criterion (x):

(i) if $s > x$, reject H_0 and accept H_1;

(ii) if $s \leqslant x$, reject H_1 and accept H_0.

The application of a rule of this sort involves a definite decision about each sample. In contradistinction to the comprehensive test rule, we may, however, apply a *partial* rule which guarantees no decision in advance. Such a rule, as advocated by R. A. Fisher, invokes only one test (so-called *null*) hypothesis (H_0). The pattern is:

(i) if $s > x$, reject H_0;

(ii) if $s \leqslant x$, reserve judgment.

To clarify the outcome of the two procedures, we may suppose:

(a) we know that a pack of cards is complete or that someone has removed the hearts, so that the probability (p) of drawing a black card is either $\frac{1}{2}$ or $\frac{2}{3}$;

(b) we formulate a rule of decision applicable to samples of n cards picked with replacement on the understanding that we reshuffle between successive withdrawals.

We have here two test hypotheses: H_0 that $p = \frac{1}{2}$ (pack complete) and H_1 that $p = \frac{2}{3}$ (hearts removed). To make a useful rule of the comprehensive type, we must choose a rejection criterion x which ensures that the probability of error is less than some acceptable limit $P_f \leqslant \epsilon$ whether H_0 is true or H_1 is true. If we label the number of black cards as s, our rule as stated above therefore signifies that the frequency of samples whose definitive score is $s \leqslant x$ does not exceed ϵ when H_1 is true and that the frequency of samples whose definitive score is $s > x$ does not exceed ϵ when H_0 is true. We cannot satisfy both conditions unless n itself exceeds a certain limit. If we require $P_f \leqslant 0{\cdot}05$, samples of 100 suffice and the rejection

criterion will be $x = 58{\cdot}5$. For samples of $n = 10^1$, we can make such a rule to satisfy the requirement that we shall be more often right than wrong ($P_f < 0{\cdot}5$); but we cannot do so for $n = 5$. There is no value of x, which satisfies for $n = 5$, both the condition $P(s > x) < 0{\cdot}5$ when H_0 is true and the conditon $P(s \leqslant x) < 0{\cdot}5$ when H_1 is true:

We may tabulate as follows the score (s) distributions for the alternative hypothesis $p = \frac{1}{2}$ and $p = \frac{2}{3}$ when the size of the sample is $r = 5$

$P(s)$ on H_0 $(p = \frac{1}{2})$		$\frac{1}{32}$	$\frac{5}{32}$	$\frac{10}{32}$	$\frac{10}{32}$	$\frac{5}{32}$	$\frac{1}{32}$
$P(s)$ on H_1 $(p = \frac{2}{3})$		$\frac{1}{243}$	$\frac{10}{243}$	$\frac{40}{243}$	$\frac{80}{243}$	$\frac{80}{243}$	$\frac{32}{243}$
Score (s)		0	1	2	3	4	5
$P(s > x)$ on H_0	1·000	0·969	0·813	0·500	0·188	0·031	0·000
$P(s < x)$ on H_1	0·000	0·004	0·045	0·210	0·539	0·868	1·000
$x =$	−0·5	0·5	1·5	2·5	2·5	4·5	5·5

Now we can evidently satisfy the condition $P_f < 0{\cdot}05$ for samples of 5, if we make the partial rule: reject H_0 if $x > 4$ and otherwise reserve judgement. We cannot then err if H_0 is false, and our statements will be false only of 1 in 32 (< 5 per cent) of all samples we meet. This view of the situation is instructive because it is one of the claims of Fisher's school that their procedures permit us to deal with small samples. So seeming an advantage dissolves in thin air, when we ask: what proportion of all samples will dictate reservation of judgment? Should it happen that H_1 is true, H_0 would be false and we should rightly reject it when $s > 4$. For the five-fold sample, however, we should then have to reserve judgment about 211 in every 243 samples. Thus we have no guarantee that the performance of the test will lead us to reject H_0 more than about one in ten times when it happens to be false.

Even if we dismiss the view that any rule of stochastic reasoning

[1] The reader who finds the rationale of the dual test difficult to follow at first reading may work out the relevant frequency distributions for $n = 10$, i.e. successive terms of $(0{\cdot}5 + 0{\cdot}5)^{10}$ and $(0{\cdot}3 + 0{\cdot}6)^{10}$. In round figures, the percentages of all samples we shall meet are classifiable thus:

$s \leqslant 5$	63 per cent if $p = \frac{1}{2}$	22 per cent if $p = \frac{2}{3}$
$s > 5$	37 per cent if $p = \frac{1}{2}$	78 per cent if $p = \frac{2}{3}$

If we accept H_0 $(p = \frac{1}{2})$ whenever $s \leqslant 5$, we can thus be wrong only if H_1 $(p = \frac{2}{3})$ is true, i.e. we shall be wrong about 22 per cent of samples we encounter. If we accept H_1 whenever $s > 5$, we shall be wrong only when H_0 is true, i.e. we shall be wrong about 37 per cent of our samples. Either way, we cannot be wrong about more than 37 per cent, and must thus be right about at least 63 per cent of the samples we shall meet in the long run.

imposes on us the obligation to state it in advance in its entirety, whence also a specification of sample size, one conclusion is therefore inescapable. Unless we set in advance a limit to sample size, a test procedure of the partial type as prescribed by R. A. Fisher carries with it no guarantee that it will lead us to reject a false hypothesis in an assignable proportion of situations in which to apply our test. How useful any such rule may indeed be involves other considerations, in particular that testing a hypothesis in this way is possible only if we can associate it with a unique sample distribution of scores definitive of events whose occurrence it endorses. Except in certain situations which arise in quality control of industrial products, it will very rarely happen that we can set up two clear-cut exclusive alternatives for a comprehensive test on this understanding. To be sure, we may often be able to set up some single (so-called *null*) hypothesis for Fisher's significance test procedure; but the restriction of our statements to rejection and reservation of judgment need have no bearing on the putative operational intention. In practice, what prescribes the choice of a null hypothesis is the possibility of defining it in the terms stated, i.e. in terms of a unique sample distribution, and this of itself has no bearing on the presumptive aim of the scientific investigator. Indeed no example of a significance test I have ever seen cited in biological literature is immune to the objection that it confers only one of two benefits: arrival at an irrelevant conclusion or at no conclusion at all.

In my parable of the card pack, we supposed that either one-half or two-thirds of the cards were black. In short, we assumed some prior knowledge to equip the discussion with the clear-cut alternatives we all too rarely meet in real work. If we drop any such supposition overboard, we may seek an answer to a less restrictive class of questions: what types of statement can a stochastic calculus endorse with respect to an acceptably small range of values within which the proportion of black cards lies? Needless to say, we then admit a frequency of erroneous statement at some preassigned acceptably low level. Such is the pattern of problems with which interval estimation deals. If we undertake such a programme, we may elaborate our definitions and recipes, as do Neyman and E. S. Pearson, in terms which are consistent with the Forward Look or we may do so (as does R. A. Fisher) in terms which sanction retrospective judgments. The two formulations respectively invoke the

concept of *confidence* and that of *fiducial probability*. In a spirited defence of the latter, which is indeed a recrudescence of the doctrine of inverse probability, Dr Yates invokes a *new law of thought*. The only reason he gives for doing so is to sustain a faith which would otherwise lead to inconsistencies. Liberal theologians have long since abandoned persuasion at this level.

The statement of a rule of interval estimation in its entirety must include the size of sample as a requisite for reliance on a preassigned uncertainty safeguard. Needless to say, it must also incorporate the end in view. In other words, it must set *acceptable* limits to the interval it endorses, e.g. that the true proportion (p) of black cards in the pack lies within the range 0·45 to 0·55. Seemingly, this frees us from the dilemma of making a quite arbitrary choice about clear-cut alternatives (such as $p = \frac{1}{2}$ or $p = \frac{2}{3}$) in the absence of prior knowledge; but the removal of the restriction imposed by the limitations of our prior knowledge carries with it a penalty. The parameters $p = \frac{1}{2}$ and $p = \frac{2}{3}$ serve both to define alternative hypotheses uniquely and to define uniquely different sampling distribution. Alas, we have no advance knowledge of relevant parameters of a sampling distribution fully appropriate to the end in view in most circumstances to which the technique of interval estimation is relevant.

This inconvenience has come into focus quite recently. Until 1950 it seemed that the peculiar algebraic properties of the so-called t-distribution met our requirements; but its claims to usefulness break down, if we demand an advance statement of a stochastic rule *in its entirety*. As I have already stated, it gives us no preview of the requisite size of sample to guarantee that the interval consistent with a preassigned uncertainty safeguard will be finite. Now the only advantage we gain by relinquishing the certainties of traditional reasoning in favour of reasoning circumscribed by an assignable frequency of error is that the outcome will be more informative. Accordingly, a stochastic procedure which can set no finite limits to what the outcome may be fails to materialize its only intelligible justification.

We are now ready to take stock of the present crisis in statistical theory in more general terms. As we use the term today, Statistical Theory covers at least four undertakings which have no common factual rationale. All that they share is reliance on a calculus of prob-

ability and the use of formally identical operations prescribed by the calculus. Experts by no means agree about the circumstances which prescribe the relevance of the calculus of probability to human experience; and different views about its relevance go hand in hand with widely different prescriptions for putting it to work. We can all agree that it has proved to be a powerful recipe for devising models in the domain of experimental physics and genetics; but its credentials are directly accessible to justification by works only in such situations. If we take a charitable view of its claims, we may also concede that it provides an intelligible rationale for a disciplinary regimen when we have to deal with instrumental errors.

Of these two accredited applications of a calculus of probability, neither the one nor the other has any factual relevance to the claims of a stochastic recipe for exploratory purposes in the biological and social sciences nor for assessing the value of testimony which the data of populations supply. A consistent justification for the former betokens a return to Platonic concepts which most men of science reject. Any future for the latter is problematical unless our successors can equip it with mathematical aids now lacking. Whether they will prove equal to the undertaking remains to be seen.

So nihilistic a view of the present content of statistical theory need not discourage the professional statistician, if prepared to explore the credentials of summarizing procedures on their own merits, and if content to advance for the usefulness of what summarizing procedures he invokes no claims other than such as are justifiable in the domain of the new *non-monetary* accountancy of large scale administration or manufacture. Nor need it evoke alarm and despondency among those with a concern for the future progress of the biological and social sciences. A tradition of instruction in the use of quasi-stochastic techniques has taken shape during our generation with consequences many of my contemporaries must now deplore. It is an overstatement to say that a contemporary university course of liberal theology is more congenial to unfettered curiosity and to the temper which declines to take dogma on faith than is the conduct of current courses in statistical theory for students of biology, sociology, and psychology in our seats of higher learning. During the past thirty years, a calculus of uncertainty has become the creed of a cult which disdains to press forward to greater certainty when greater certainty is indeed attainable, and when nothing short of

certainty constitutes a useful addition to the enduring corpus of scientific knowledge.

Accordingly, the outcome of controversies which have brought into the open the suspicions I have voiced should be salutary. Since we now see that the College of Cardinals cannot agree among themselves about the rubric, we should be ready to return to a less authoritarian tradition of research. The glamour of statistics has hypnotized a younger generation into asking questions which admit of answer only in terms of averages, and only so, if we restrict the form of the question to the requirements of certain rule of thumb procedures. That the rationale of these procedures is highly controversial may henceforth encourage young investigators to ask in what circumstances questions which admit of answers only in terms of averages are profitable questions to ask. This may well drive them back to the exercise of their own creative resources. If so, biology, sociology and psychology will reap immense benefit from the dissolution of the Statistical Court of Appeal. It will be less easy to attain high academic honours by painstaking performance of calculations referable to intrinsically uninformative data; but a career of research will once again promise the excitement of mental adventure and the prospect of discovery at the end of the trail.

7

SCIENCE AND ITS SOCIAL FUNCTIONS[1]

WHEN it was fashionable to talk about Art for Art's sake, Samuel Butler put the question *What is Art to have a sake?* In the same temper I prefer to discuss the social functions of science in terms of the usefulness of the scientific worker to the society of which he is a member. So anything I want to say may well come within the legitimate scope of the title of the address which Kathleen Lonsdale has agreed to deliver. I can merely hope that it will not unnecessarily anticipate what she will say about the many issues on which she will doubtless express my own convictions more convincingly if less light-heartedly.

In my sixties, it is increasingly difficult for me to find a unitarian formula for the trinitarian diversity of now respectable thinking called science, embracing, as it does, the Nautical Almanac of the unchanging periodicities of astronomy, the Cookery Book recipes of the prospective inquiries we severally call chemistry, laboratory physics and physiology in the same *galère* as the Doomsday Book pronouncements of the retrospective disciplines such as geology, archaeology and evolutionary biology. If I could indeed find one, it would certainly provide no *niche* for Hayek's economics nor for Freudian psychology; and it would overtax my ingenuity to accommodate the recklessly unreliable extrapolative exploits of the Meteorological Office.

By the same token, I can concoct no simple formula to encompass

[1] Delivered to a Symposium on Science and Society jointly organized by the Institutes of Biology, Physics and Chemistry under the chairmanship of Professor P. M. S. Blackett, F.R.S. (Nobel Laureate).

what we may properly call the social usefulness of the scientific worker. I do not discard the old-fashioned belief that different sorts of such usefulness may have an enduring value; but it seems to me that intelligible criteria of what is more or less useful in a particular and temporary social context depend on its own character. On this understanding, I should assuredly decline to define comprehensively what I do regard as the social usefulness of the scientific worker; but any formula to which I can personally subscribe would embrace at least two objectives.

Bacon expressed one as the endowment of human life with new powers and inventions. Lucretius hints at the other, when he spoke of the deliverance of mankind from the terror of the gods. The collective effort of scientific workers can enrich the life of mankind with the means of eliminating hunger, the hazards of the seasons, disease and irksome toil. The non-authoritarian habit of reasoning allied to a lively and tolerant curiosity essential to the continuance of this collective effort can also reinforce social pressures for the elimination of superstitious cruelties, as for instance witch-burning in the seventeenth century. It does not seem to me wise to regard one benefit as intrinsically more important than the other; but I deem it prudent to regard one or other as more important in a particular historic milieu. Needless to say, the story runs a different course in England and in Scotland, in France, in Germany and in Italy; but I shall ask you to recall only a few episodes of the English scene.

Before doing so, let me forestall a misunderstanding. I am not discussing the motivation of the individual research worker. If one concedes that this may, and commonly does, have little connection with the social usefulness of the outcome, it does not follow that a research worker, as such, is any the better because indifferent to his or her obligations to fellow citizens, still less that scientific advance is conspicuously rapid when such indifference becomes a cult. The history of science bears testimony to the contrary.

Roughly three centuries have elapsed since the days of the Italian *Lincei* and the gatherings which gave birth to the Royal Society in England and the Academy of Paris. Thenceforth, we can first speak of scientific workers as a social group. To do so, implies their recognition of responsibilities to one another in virtue of their common interests, for instance with respect to publication, discussion, demonstration and other means of intellectual cross fertilization.

What is more relevant to my theme is that all occupational groups from time to time enter into united fronts with others to promote objectives propitious to each; and an enduring recognition of wider communal responsibilities may emerge in the build-up of such temporary alliances.

When the Royal Society drew up its *Heads of Enquiries*, in the Newtonian age, the English Establishment, in active alliance with the Mine-owners and Merchant Princes of Maritime Trade, was on congenial terms with a Laodicean tradition of Broad Churchmanship with seemingly little reason to fear the consequences of the Clarendon Code. In the *Novum Organum*, Lord Chief Justice Verulam had long since laughed out of court what Bishop Spratt called the disputatious way of the ancient philosophers. In Cambridge, Aristotelian physics was as dead as a doornail; and dissenters, such as the early Quakers, were still pathetically comic. Newton's unitarianism basked in the laxity of a Restoration with only the uncomfortable prospect of Popish plots to disturb the equanimity of a Pepys, before Newton himself sat for Cambridge in a House which partially reaffirmed the religious toleration of the Protectorate. Otherwise, one may say that the ideological climate was favourable to a confident curiosity and an adventurous inventiveness with no prevision of other times which would recreate the Galilean contretemps with clerical authority. Witch-burning apart, the Lucretian motif was a still small voice.

The Newtonian tie-up with maritime enterprise outlasted the Board of Longitude and the voyages of Captain Cook; but new alignments, with a devastating impact on the Establishment when eighty Fellows of the Royal Society resigned in 1833, took shape during the period of a second industrial revolution which began with the manufacture of sulphuric acid in England and Scotland during the middle of the eighteenth century. Initially, only those who were outside, or on the periphery of, the Establishment, as were Priestley, Henry, Dalton, Davy, Joule and Faraday, allied themselves with the new manufacturing *bourgeoisie* socially and, as is conspicuously true of Priestley, politically; but there were rich rewards for the inventive ability of a Kelvin at the end of a century celebrated by the Great Exhibition of 1851. A new Establishment had then come to terms with a largely nonconformist personnel of industrial owner-managers.

To speak of an alliance between the opposition to the Establish-

ment and the new *bourgeoisie* in the first half of the same century is a half truth, if we fail to take stock of the role of master craftsmen such as Murdock—or of James Watt before he made good. The lectures to Working Men in the Royal Institution still rank as our only epics of scientific writing; and they are redolent of an appeal to motives other than material gain. The revolt which coincided with the issue of Babbage's famous tract on *The Decline of Science in Britain* and the start of the British Association had roots in the genial complacency of the Newtonian age. When the newly founded godless College in Gower Street was as yet less an educational instrument of the new technology than a forum for the new humanitarianism of Jeremy Bentham, the teaching of science, never quite honourably at home (then or now) in Oxford, was at its lowest ebb in Cambridge since the time of Isaac Barrow and Wallis. Meanwhile, the Test Acts were still in force in the two older seats of English Learning, and excluded such men as Priestley, Faraday and Dalton from access to university instruction. In Britain, Scotland, where the Act of Union sidestepped the terms of reference of the Clarendon Code, supplied the bulk of university graduates trained to meet the pressing needs of the time.

In all this, there was at first no prevision of the emergence of the Lucretian motif of the social function of science—so imperative in the France of Mersenne and of Richelieu, so articulate in the *École Polytechnique* when Laplace, Legendre and Lagrange were collectively the mouthpiece of an ascendent anti-clericalism; but there were already ample premonitions of the great ideological struggle of the nineteenth century for the secularization of education. In the England of Albert the Good, clerical intransigeance, which obstructed legislation for the compulsory liquidation of illiteracy till the passage of the Balfour Act in 1902, was enlisting an army of adherents for a new United Front in which men of science were to play a leading role.

Its pioneer was John Stuart Mill, whose pedestrian but more eclectic reaffirmation of the Baconian gospel might well have escaped notice if Bishop Wilberforce had not ill-advisedly dropped a grenade into the nimble grasp of Thomas Henry Huxley just when the demand for the Repeal of the Test Acts was a pivot of national controversy. The Evolutionary debate thus became the tocsin of a new Lucretian evangel. As expounded by T. H. Huxley and

Tyndall, science was organized common sense; and it was the trademark of the man of science to take nothing on trust. With nothing to lose by assuming a novel social function as the bulwark of enlightened mankind against clerical authority, the Establishment soon relapsed into genteel aloofness to the maintenance of Britain's not as yet recognizably precarious supremacy as the world's workshop.

Circumstances inescapably tailored the new ideology of the Establishment to the prevalent doctrine of *laissez-faire*, when the Test Acts ceased to be an issue, and one could thankfully dismiss politics as irrational. As the rationale of the new rationalism, science must needs be above politics in the tepid climate of Edwardian belief; and World War I did little to jolt the Establishment out of this genteel aloofness to the world's work. More ample endowments from Government and Industry were forthcoming to forestall another situation in which Britain's provision for research would compare unfavourable with that of its competitors. So politics were taboo till a trade crisis of countries in which the capitalist economy was dominant faced the challenge of the first large-scale planning projects of the Soviet Union.

To many of my own generation, still in their late thirties or early forties throughout the Great Depression, Huxleyan rationalism had forfeited the charm of novelty. To us, it was now abundantly clear that unrestricted private enterprise could no longer offer the prospect of an expanding economy capable of exploiting and advancing scientific knowledge in the interests of mankind, that the new powers and inventions which scientific discovery had placed at our disposal could immeasurably raise the standard of life at least in the Western—our own—half of the world if the community planned to that end, and that scientific workers accordingly had a special responsibility as citizens to participate in the national movement of which the Welfare State is the offspring.

At that time, it would have been hard to enlist for an occasion such as this symposium a man of science of the age group to which I now belong without the prospect of a sermon on the text that young scientific workers should keep clear of politics, meaning that they should take their views from the *Daily Telegraph* and entrust the destiny of Britain to Stanley Baldwin. However, the mental climate was favourable to the proponents of social planning and to the plea

for a programme of national education attuned to a realistic recognition of the social benefits which science can confer. Among my own generation, many who had hitherto been cynically aloof to social issues knew which side of their bread had no butter when the slump cut off educational grants, resulted in a vast reduction of the value of capital investments held by the Foundations for dispensing research funds and shut the door to new openings for science graduates in industry.

Today, a different situation confronts us. Of social planning, we may say what Augustine said of the City of God: *petant aut non petant venire habet.* In short, we are moving into a more planned society, whether we like it or not. What is of overwhelming topical significance is that scientific work, to an extent never before in the last three hundred years, has emerged from World War II in a close alliance with—and bondage to—the military machine at a time when scientific discovery has equipped mankind for the first time in the 25,000-year-old human story with a swiftly sure means of destroying all animal life on this planet.

At such a supreme crisis in the history of mankind, it would be pleasant to record that the middle generation of scientific workers realize that they have a far greater responsibility to their fellow citizens than ever before. I earnestly hope that a gathering such as this testifies to a disinclination against relinquishing this responsibility to the generation of Bertrand Russell, Toynbee or J. B. Priestley; but if so, why are there none of a much later vintage than my own generation on this platform? At a time when the Left press and Right-wing politicians chant in unison the praises of science and the need for training more scientific workers, there are admittedly some temptations to complacency; but no scientifically trained person can pardonably fail to realize that we now face the choice between till lately unimaginable prosperity for all mankind and the destruction of the human species—including the Lucky Jims of the Atomic Power Establishments.

In this milieu, British scientific workers have as plain a duty to themselves and to their families as to other citizens. That duty is to confront the paranoid procrastinations of politicians of the Western Bloc with a body of public opinion attuned to the cataclysmically calamitous consequences of the delaying tactics of traditional diplomacy while new entrants in the nuclear weapons race daily

heighten the potential of despair. While there is so little likelihood of living to witness the continued existence of any variant of the human ecological system, the luxury of disagreeing about the merits and defects of democratic socialism, American free enterprise and Communism is like the christenings in the *Importance of Being Earnest*, a trifle premature. The only party now entitled to the uncompromising loyalty of sane people is the Party of Human Survival.

At all times a great peril, it is less clever to find rational grounds for despair than reasonable grounds for courageous action; and there is still a slender strand of hope if the party of sanity and human survival can mobilize the masses now disillusioned by the warmongers of the two great party machines. Having still a faltering foothold in hopefulness, I therefore think it may not be wholly fruitless, if admittedly premature, to speculate about the usefulness of the scientific worker in the unlikely event of human survival A.D. 1968.

What will then be the supreme social function of the scientific worker in a world-wide society which may henceforth enjoy so abundant a life? In terms of the Baconian motif, I associate myself with every sentence, word, syllable, consonant and vowel of Professor Blackett's courageous appeal at the British Association. In their own interests, scientific workers have everything to gain by supporting the allocation of generous loans from the exchequer to enable the poverty-stricken peoples of the backward communities of our colonial system to import the good things which scientific ingenuity can create. None the less, the enormity of the world-wide prosperity within our grasp may well encourage us to ask in what sense the Lucretian motif is also relevant to our usefulness to society.

Was my own generation too easily convinced that science had fought its last great campaign against authoritarianism under the leadership of Thomas Henry Huxley? Is it true that scientific education is today a bulwark against arbitrary intellectual authority? Is it remotely true that scientific education, as we know it, teaches us not to take too much on trust? Is it true that the scientific attitude, so defined, has ever been socially articulate except in the initial stages of formalizing the content of a new curriculum of scientific instruction? Does the record of the medical experts of the Concen-

tration Camps justify the claim that scientific training is a prophylactic against senseless cruelty?

But need I bring in Buchenwald? Recently, Sir David Eccles[1] said that only a handful of pacifist cranks pooh-pooh the unanimous counsel of the top-level scientists on whom the Government can rely for an expert opinion about the dangers of massive retaliation. I do not pretend to know what Sir David Eccles aimed to convey unless it be that the scientific worker with a good enough prospect of being on the New Year Honours List can be as adept at lying as lawyers. In that event, you and I should now cease to luxuriate in the contemplation of an agreeable self portrait of the scientific worker as a knight errant in tireless and selfless quest of the Holy Grail of truth for its own sake.

I hope that a younger generation of scientific workers is seeking an honest answer to the questions I have last stated; but I could wish that my own contemporaries were less complacent about the need to ask them. To be sure, there exists in Britain a so-called *Committee of Science and Freedom*; but its main concern is to attack limitations imposed on scientific discussion in the birth-pangs of the new type of Society emergent in Eastern Europe and in all Asia north of the Himalayas. That such impositions exist I do not deny. That it is the rightful concern of individuals, as individuals handicapped by them, to resist them, I concede in the hopeful assurance that arbitrary authority has never yet entirely quenched the impulse to know, to utter and to argue freely according to conscience. That sympathy overtly motivated by political hostility to the régime of the Soviet Union and the new China will help them much is a highly dubious claim.

For my part, I regard it as more seemly to deal with the beam in Birmingham than to denounce the mote in Moscow. It does not call for great courage to indulge in passionate denunciations of someone as silly as Lyshenko from the safe distance of London, Liverpool or Leeds. What is far less popular and more appropriate is to assert that an increasing measure of social prestige now conceded to specialization in the natural sciences coincides with an increasingly authoritarian temper of instruction and an increasingly complacent attitude towards the need for an educational reorientation to correct it. With more time at my disposal, I could illustrate this new auth-

[1] Then a Cabinet Minister.

oritarianism at many levels. It here suffices to recall the formidable and increasing army of young biologists and psychologists who have lost track of any questions of practical or theoretical interest in a pathetically irrelevant anxiety to put their data through the mill of statistical tests based on highly controversial assumptions which they themselves have not examined.

If a new authoritarianism is in the ascendent, it is a disservice to blame it petulantly on Communism or Communists. Its sources are diverse. Partly, it is the outcome of the increasingly large scale of scientific team work, West and East alike. Partly also, it has come about because our educational techniques have not kept abreast of the vast increase of knowledge during the past half century; and one reason for this is an all too commonly contemptuous attitude of research workers to what teaching responsibilities they may have and to those of us who address ourselves to a wider audience in the tradition of Davy, Faraday, Tyndall, Huxley and Ball. For my part, I regard this widening gap between what a few leaders know and what an army of camp followers take for granted as a great peril to the cultural values to which scientific workers pay lip service. The only period of human enlightenment comparable to our own had its focus for six centuries in Alexandria. What reduced Alexandrian civilization to a ruin was not Caesar's invasion, nor the monks of St Cyril, nor the Moslem armies. It had in itself the seeds of its own decay, because no society is safe in the hands of so few clever people.

We shall not reinstate the lost lucidity which has made science a bulwark of intellectual freedom in the past unless we resume the task of the French National Assembly at the time of the introduction of the C.G.S. system. There is now need for an overdue and more radical reform of Human Communications. This can be the next social adventure to which scientific workers can make a unique contribution only if human beings survive; and human beings will survive only if we make some immediate changes in our linguistic habits. For *Nuclear Deterrent*, we must learn to say *Incentive to Annihilation*, for *Civil Defence*, we must learn to say *There is now no Hide-Out*, for *Massive Retaliation* we must learn to say *Universal Suicide* and for *Conventional Arms* we must learn to say *Red Herrings*.

8

ASTRAGLOSSA
or
FIRST STEPS IN CELESTIAL SYNTAX

UNTIL human beings have instituted some system of world government to guarantee law and order on our own planet, no reflective person can entertain a favourable prognosis with respect to their continued survival as a species in the age of Atomic Power. On the threshold of World War III one can therefore forecast the further progress of modern science with little confidence to justify either the hope of establishing communication with our celestial neighbours, if such there be; or of testing their possible existence by interplanetary travel. Nor need the unlikelihood of achieving this goal evoke sentiments of alarm and despondency. Before we have proven ourselves worthy to occupy our own planet we may well dread the consequences of publicizing our existence to beings conceivably intelligent enough to forestall any depredations we may contemplate in other parts of the solar system. When we reflect upon the trail of still unsolved problems of race relations in the wake of the slave-raiding bible-punching buccaneers of the first Elizabethan era, we may permissibly share Dean Swift's doubts about the fitness of man to undertake indiscriminate exploits in colonization.

None the less, the possibility implicit in the invitation extended to me by the British Interplanetary Society is a challenge to cerebration, if only for its entertainment value to myself. Nor do I feel guilty in such self indulgence. When too many miners were on the hunger march and too many men of science secluded in their own towers of ivory, it might be necessary to emphasize the material uses

[1] An address delivered to the British Interplanetary Society and reprinted from its Journal, November 6, 1952.

of science, as I have done in *Science for the Citizen*. With the foundations of a Welfare State truly laid and the term operational research perpetually on our lips, there is now a useful niche for useless speculation which brings us face to face with eternal verities uncongenial to the authoritarian temper of our times. In any case, our topic raises issues which may give us deeper insight into the education of our co-planetarians.

Those who recall the literal Greek derivation of the word will not need the reminder that my theme is an exacting exercise in *semantics*. It is moreover an exercise which bears on a special type of teacher-pupil relationship, if we envisage, as we must, the likelihood of some measure of reciprocity in the accomplishment of the end in view. Too many people feel about semantics what others feel about posterity. I do not refer to the reflection that posterity is round the corner. I mean that it has not done much for us hitherto. Any puritans disposed to regard our *jeu d'esprit* as deplorable waste of time by an individual who might otherwise be more usefully employed in serving that great Leviathan, the State, may therefore relax like the lady in the Chinese proverb, when faced with worse than death. Our undertaking will be truly serviceable, if we are thereby better able to understand what semantics can do for self-education by giving it a novel, difficult and challenging job of work to do.

We are here talking about a possibility within the conditional framework of the hypothesis that there exist extra-terrestrial beings, and that communication with them is realizable. At the outset, this arbitrary axiom calls for more explicit statement. In the light of existing knowledge, we must first postulate two subsidiary assertions:

(a) our extra-terrestrial neighbours (E.T.N.) either have somatic receptors for some range within the complete spectrum of radiant energy or have recording instruments for detecting the same;
(b) both they and ourselves have the means of transmitting radiant energy detectable as such to both of us.

That we are now able to talk abour radar echoes from the moon's surface gives the possibility last stated more prospective plausibility than the Wellsian romance about its first earth visitors. Still, the foregoing postulates, though necessary, are not sufficient conditions of the practicability of interplanetary communication. Besides bio-

mechanical assumptions, we shall have to introduce others at an interplanetary *scholastic* level. Most important of these concerns how we can initiate *long distance* communication with beings with whom we presumptively share no idiom of discourse. This will be possible only if there exists a common field of concepts and of experience to endow our signals or theirs with meaningful content. This condition, the existence of a *common field of semantic reference* (C.F.S.R.), is the one which will necessarily engage our prior attention.

On this understanding we are free to suppose that we ourselves take the initiative or contrariwise. The alternative provokes the imagination to undertake no exacting effort except in so far as a clearer view about how we should proceed to teach our E.T.N. may help us to anticipate more clearly how we may be able to learn from them. Though some measure of reciprocity in the teacher pupil relation would appear to be inevitable, we shall thus keep the ensuing discussion at a more general level, if we regard our task as instructional.

In defining the initial C.F.S.R., my own limited powers of imagination can conceive of only two categories of common concepts and common experience. That people who use multitudinous scripts and languages now employ the same Hindu-Arabic system of numerals reminds us that *number* is the most universal concept by reference to which we can readily establish one-way communication with the first human beings who tried to communicate at a distance through the medium of writing. That the calendar is a major preoccupation of the earliest scripts likewise forces on our attention that *celestial events* will likely prove to be the only common topic of experience with our E.T.N. in the initial stages of establishing any sort of communication with them. With due humility towards our limited speculative faculties, we may thus state the axiom of the C.F.S.R. as follows. *Number will initially be our common idiom of reciprocal recognition; and astronomy will be the topic of our first factual conversations.*

As we know, all early numeral systems embody the *principle of repetition* which has come down to our time in the first three cyphers of the Roman and of the Chinese battery of number signs. In a manner of speaking, the Hindu-Arabic symbol for unity is a vestige of its original function, i.e. to visualize marks made on a tally stick, on a stone or in sand as an *aide-mémoire*. What then is the compelling

necessity for any such *aide-mémoire*? Surely the answer is simple. At a low level of social organization, human beings have little, if any, use for symbols to make a permanent record of static objects easily enough enumerable in one and the same setting. On the other hand, we cannot count *events* in this way. Time flies; and the memory of the wisest old man or the wisest old woman of the tribe can no longer keep in step with it, when more settled ways of life create the social necessity for a priestly caste entrusted with the custody of the calendar. There is therefore some substance for the supposition that other beings able to contemplate the firmament and to organize settled life in conformity to the *dictates of seasonal change* will share with us the abstract concept of number.

This consideration raises a query of minor interest: can the putative inhabitants of Venus indeed contemplate the firmament through the cloudy atmosphere which enshrouds their world? Since we cannot answer this question, it may be better to talk about Mars, and to speak generically of our extra-terrestrial neighbours as Martians, which sounds less like an excess profit tax than E.T.N. Henceforward, unless otherwise stated, we shall therefore speak of Martians in a figurative sense, though there may be good enough reasons for referring explicitly to the inhabitants of Mars as such at a later stage. To dramatize my theme, I should prefer it otherwise, because the Venus Cycle occupies such an important status in the Mayan calendar from which we shall get our next cue.

Though we cannot communicate with our Neolithic forbears, they themselves can communicate with us through the mists of time. Thus the archaeologist has a special contribution to the agenda of our symposium. For our purpose the script of the cultures of Guatemala and Montevideo, not very long extinct when white men landed in America, will be specially instructive because number symbols and astro-calendrical hieroglyphs circumscribe its content. It has therefore both components for our first interplanetary colloquia, and little if anything more. Within the framework of our axiom, it will supply us with all the ingredients for *teaching the Martians how to talk with us by visual aids.*

At this stage, we need say nothing about the astro-calendrical hieroglyphs except to mention that they confront the imagination with no challenging issues other than those faced and solved within limits consistent with our theme by the late Otto Neurath's inter-

national dictionary of Isotype and the system of world-wide *Semantography* devised by an Australian chemist, Bliss, for speeding up the education of preliterate communities.

Here, as possibly earlier, someone may ask: why I do not ask what clues we may gain from: (a) how the isolated shipwrecked mariner learns the lingo of the North Caroline Islands; (b) why I say nothing about what we may conclude from the way in which children learn to talk? The answer to the first question is simple. While the sailor can point at things in the earliest stages of associating a new sound symbol with them in a framework of *direct* communication, our major preoccupation in the long-distance situation we here conceive will be to devise a technique of how to point at things. As regards the second query, I should like to be able to record that educationists are likely to travel on any terms other than as passengers in our adventure; but I hesitate to take them on board in an executive capacity. The sad truth is that professional educationists and their unofficial opponents are, broadly speaking, divisible into two categories: (a) those who regard it as their main business to establish the ineducability of the pupil by recourse to factor analysis; (b) those who maintain that trying to teach a child anything when it should be developing its so-called personality is an extremity of cruelty overdue for incorporation in the penal code administered by the Law Courts. The only unmentioned clue that I regard as specially relevant to our theme I shall merely refer to *en passant*, viz. what we now call programming, i.e. the syntax of the language in which we transmit orders to the new electronic computing machines.

With this digression behind us, we can get back to Guatemala, recalling the essential feature of the Mayan numeral system. With one exception, the ingredients are dots and dashes laid out repetitively in a certain sequence at conventionally prescribed intervals. Thus it has three abstract components *iteration* (1), *rank order* (r) and *gap* (g). It is not necessary to explain at length that all these are translateable in a medium of interplanetary communication which may be *initially* one-dimensional, i.e. temporal. The exception mentioned above refers to the fact that the Mayan system had a zero symbol on all fours with the astro-calendrical pictograms. Initially, this will be unnecessary. Indeed, the zero concept may require elaborate exposition *pari passu* with the meaningful conveyance of our number symbols, though easily enough explicable

(*vide infra*) in a factual context. At any level of factual communication, as when we begin to associate numbers with events, we shall need to have calendrical hieroglyphs, i.e. signal patterns for concrete celestial entities in the C.F.S.R., to convey the meaning of Venus or the Sun, day and night, etc. In contradistinction to stroke or dot which I shall briefly call *dashes*, we shall thus need a special battery of signals each recognizable as a *Gestalt*; and I shall call these radioglyphs or *flashes*.

The basic elements of our first vocabulary will then be dashes (*d*) and flashes (*f*); but our use of them will depend on a system of celestial punctuation already suggested, viz. rank order (*r*) and interval (*g*). If by chance this conception is not clear, we may here set our Martians an entrance examination by repeating a signal pattern (*S-P*) to convey an identity in first steps towards a more economical numeral system, easily intelligible if we employ Roman script, viz.

$$I + II + III = VI.$$

In the flash, dash, iteration, rank and gap symbolism of our signalling system we might set out in two stages this identity with *its interpretation* as follows:

THE ENTRANCE EXAMINATION

$$\text{I} + \text{II} + \text{III} = \text{VI}$$

(i) $1..F_a..1.1..F_a..1.1.1..F_b..1.1.1.1.1.1$

(ii) $1..F_a..1.1..F_a..1.1.1..F_b..F_s..F_b..1.1.1.1.1.1$

The translation is a rather low-grade intelligence test for this audience, though not necessarily the first task we should assign to Martians about whose mental processes we, as yet, know nothing. We interpret the unit gap by a signal full stop, i.e. unit time-interval in our signalling system, as indicative of the concept of iteration, and the numbers as such by dash-iteration. We interpret the double time interval as the announcement of an impending operation or change of our symbolic conventions. In this set-up the flash F_a signifies the operation of addition, the flash F_b the relation of identification and the flash F_s as the introduction of the cypher symbol for *six*.

In the assumed role of teacher rather than pupil, we are now ready

to write on the blackboard, if successive repetitions of an appropriately simple semantic pattern have elicited the interest of the extra-terrestrial class. How we can do so will be the theme of discussion at a later stage. Here we shall assume that the class is attentive. We then reach a cross-road. Shall we start the school day with a lesson in arithmetic or with a lesson in astronomy? To weigh the implications of this decision fairly, we must first say what we mean by a lesson in astronomy. This presupposes some preliminary preparation by the teacher, in particular two tasks which we must delegate to the astronomers on the panel of experts working on the agenda of our symposium. We must first ask them to compile a day-to-day cross reference calendar of light and darkness and seasonal conditions for Mars (here used figuratively) and our earth. To do this we shall have to fix a zero of Martian longitude identifiable to ourselves as such by some prominent feature, e.g. the *Syrtis Major* or the *Sinus Meridiani* of Mars *sensu stricto*. It will not necessarily be an obvious choice from the viewpoint of the Martians who (we can only hope) will be able to identify it as such by repetitive receipt of a flash coincidence with *local* noon so defined at intervals of one Martian diurnal cycle. We are then ready for a second lesson. From our cross reference calendar we choose some celestial event such as the occultation of our own moon or a conjunction of earth and Venus as seen from Mars at a given moment of Martian time at zero Martian longitude. This is the D-moment of our next sequence messages. The lesson then signifies sending one and the same flash accompanied by x dashes at x time-intervals before D-moment, $(x-1)$ dashes at $(x-1)$ time intervals before D-moment, and so on. We could add the zero concept to the lesson as light-weight by substituting a particular flash for the now vacant dash (0 time intervals before D-moment) in the signal to synchronize with the event.

Going about our self-assigned task in this way is conceivably open to one objection. To begin the school day with a lesson in astronomy has the great educational disadvantage of trying to teach two skills in one context, i.e. to identify the number concept and to label the historical event. What if our bright Martian pupils, not as yet aware of its source, conclude that the signal pattern associated with the celestial event is a new or hitherto unrecognized regularity of nature? Since we cannot answer this question, it will not greatly com-

promise the consistency of any entertaining conclusions we may next proceed to formulate, if we now decide to divide our curriculum into a fresher course on number-lore and a sophomore course on star-lore.

Our freshman course will have two objects: *first* to convey the suggestion that we are indeed issuing signals; *second*, to introduce a system of enumeration sufficiently compact to answer the purposes of the sophomore course which follows. One way of accomplishing the first while making the task of accomplishing the second more easy suggests itself, if we recall a device almost certainly contributory in large measure to the remarkable efflorescence of the theory of numbers in India during the first two centuries of our era. The various Figurate Numbers, e.g. the infinite number of families of infinite series of which the triangular number series is the two-dimensional representative, constitute a pool of exercises in simple rules equally suitable for wall chart use in a Manchester schoolroom or signal transmission to Mars.

To exhibit the build-up of the triangular numbers, we need only two flashes F_a (+) and F_b (=) already used; but we might advantageously employ a new battery $F_{r \cdot d}$ meaning *this is an example of the rule D*. Starting with the build-up of the natural numbers our code would yield:

$F_{r \cdot 1}..1..F_a..1..F_b..1.1$ $(1 + 1 = 2)$,

$F_{r \cdot 1}..1..F_a..1.1..F_b..1.1.1$ $(1 + 2 = 3)$,

$F_{r \cdot 1}..1..F_a..1.1.1..F_b..1.1.1.1$ $(1 + 3 = 4)$.

The build-up of the triangular numbers would be:

$F_{r \cdot 2}..1..F_a..1.1..F_b..1.1.1$ $(1 + 2 = 3)$,

$F_{r \cdot 2}..1..F_a..1.1..F_a..1.1.1..F_b..1.1.1.1.1.1$ $(1 + 2 + 3 = 6)$,

$F_{r \cdot 2}..1..F_a..1.1..F_a..1.1.1..F_a..1.1.1.1..F_b..1.1.1.1.1.1.1.1.1.1$ $(1 + 2 + 3 + 4 = 10)$.

By suitable manipulation of the intervals in the flash *Gestalt* we can give our battery of instructional symbols ($F_{r \cdot d}$) a dual character. Thus they will have flash symbols already introduced, whence we can rewrite trigonometry for the Martians by recourse to definitions in terms of infinite series.

We need not necessarily wait till the stage at which we have laid down the rules of our secondary system before we enter the domain

of factual discourse. As stated, a system of number symbols based on the principle of repetition would suffice, if supplemented by a single specific radioglyph for the announcement of a particular celestial event. We should then have introduced the *substantive* into our vocabulary, which now consists of the following components:

(a) Primary numbers (e.g. 1.1.1),
(b) Cyphers (e.g. F_s),
(c) Operators (e.g. F_a),
(d) The Copula of identification (F_b),
(e) Indicatives (*this is the rule*),
(f) Substantives (heavenly occurrences).

The transmission of our first celestial news announcements presupposes as a component of its indicative content the choice of a suitable fixed time-interval to give meaning to (x), $(x-1)$, $(x-2)$, etc., units of time before the D-instant. The proper choice of it must anticipate a contingency not explicitly stated in our preliminary axioms. We have postulated that our Martians have means of detecting radiant energy in some range of the complete spectrum. If so, they may possess knowledge of the material composition of the stars definable in terms of line absorption. This opens the possibility of associating the concept of number and duration with the concept of matter in its several elementary forms. Besides celestial events in the ordinary sense of the term and possibly visible local events such as the recession of polar ice caps or changes of the canal system of Mars (*sensu stricto*), our C.F.S.R. may therefore embrace the chemistry of the stars, and thence by stages to the composition of the crust of our respective worlds.

To enlarge the section devoted to substantives in our radio-lexicon in this way, we shall need to remind ourselves that our own time-unit for specification of wave frequencies is an anomalous survival of the Babylonian numero-calendrical lore. We shall have to scrap the *second* in favour of some division of the Martian diurnal cycle or sidereal year consistent with the base of our common secondary numeral system, and preferably referable in some simple way to what we now regard as a universal constant, viz. the speed of light.

If we entertain the possibility that our Martians have advanced as far as ourselves in the science of astronomy, we may add two other abstract concepts to our vocabulary for which we shall need two

new radioglyphs. We know the relative diameters and masses of the planetary system. If we select any one planet to specify a unit of length and mass, the association of the appropriate signal for unity with the name flash for the standard planet and the association of appropriate number signals with name-flashes for other planets (and the sun) should then suffice to provide the ingredients of a semantically adequate interplanetary news item. If we have also established reciprocal communication at this stage, it may then be possible to exchange new information about the structure of the crust of Mars and of the earth.

It is indeed unlikely that we should progress very far in our sophomore course of astronomy or even in our fresher course of arithmetic without such reciprocity. Thus we have slipped in an assumption we have not as yet justified. Can we indeed discover a technique to elicit information to the effect that the teacher has the attention of the class and that the pupil understands what the teacher is trying to convey? Evidently, we cannot, if we confine ourselves to a monologue of simple assertions, no matter whether such assertions embody rules of enumeration or descriptions of the common field of our experience.

Our hitherto unmentioned problem, and one which we must face at an early stage if only to sustain our own morale as teachers, is not one to which the shipwrecked mariner or the educational theorist can contribute much, though perhaps a fuller case history of Helen Keller than any that has come my way might do so. That we must face it is evident, if we recall the morphology of our language so far conceived within the straight jacket of simple affirmation. You will notice that it has no category of pronouns. It will be easy enough to slip in a flash for *it* (F_i) and for *they* (F_t); but our teaching lacks the personal touch, till we can introduce the antithesis *we-you*. This possibility emerges so soon as we establish reciprocity by repetitive association of an *our* (F_w) flash with a question in juxtaposition to a *your* (F_y) flash with the appropriate reply; but the identification presupposes the solution of a syntactical problem, the distinction between affirmative and interrogative statement. This will be our next task.

First, we may recall a joke of Edmund Gosse[1] at the expense of

[1] *Itylus* begins with two vocatives which might as well be imperatives:
'Swallow, my sister, oh, sister swallow.'

Swinburne. In its beginnings, as it may well be in the beginnings of all human speech, the vocabulary of the ship-wrecked mariner might likely consist of three main components: *substantives* (stick, shell), *imperatives* (push, lift), *vocatives* (you and any personal name) with the several functions of identification, eliciting action, engaging attention. In the beginnings of interplanetary communication, as here conceived, we shall have no imperatives *sensu stricto* and repetition of signals will be our only means of eliciting information. We can lay down for ourselves or for the Martians the outlines of a syntax which is essential to rapid progress in the shipwrecked mariner's self tuition and an indispensable prerequisite to reciprocity of interplanetary communication, only if we can prescribe a means of distinguishing:

(i) affirmation, negation and interrogation;
(ii) assent, denial and doubt.

This will be our initial problem of celestial syntax, as I here conceive the programme; and I do not think that it is difficult to accomplish a possible solution of it at a very early stage. We have labelled a class of affirmations by the flash $F_{r\cdot d}$ ('the rule for the dimension *d* of this family of series is'). This is simply an affirmative label for a signal sequence announcing the build-up of a number series. Let us now transmit the same signal with two modifications. We substitute a new preposited flash $F_{n\cdot d}$ for $F_{r\cdot d}$ and insert an erroneous term in the series in juxtaposition to the foregoing message. By repeating the lesson, the class infers that $F_{n\cdot d}$ means 'this is *not* the rule' . . . , etc. This flash like $F_{r\cdot d}$ will have two components, a radical and a qualifier in terms of the Chinese ideogram; and the radical (F_n) will have the function of its abstract content, i.e. the negative particle.

Now we can play with this theme at many levels as follows. We

A FIRST LESSON IN CELESTIAL SYNTAX

Affirmative

$F_{r\cdot 1}..1..F_a..1.1..F_a..1.1.1..F_b..F_s$

Negative

$F_{n\cdot 1}..1..F_a..1.1.1..F_a..1.1.1..F_b..F_s$

Interrogative

$1..F_a..F_q..F_a..1.1.1..F_b..F_s$

can drop out of our message the affirmative $F_{r \cdot d}$ and substitute a new dual flash $F_{q \cdot x}$ (*what* is the *xth* term?) for any member of the number series; and have now found a basis for the concept of interrogation, e.g.

$$1..F_a \quad F_{q \cdot 2}..F_b..1.1.1 \qquad\qquad 1 + ?(x_2) = 3.$$

Again we conceive F_q as the radicle of a Chinese ideogram, so that we can separate the abstract concept (F_q) of query from the particular context. Thus we may symbolically write $F_{q \cdot x}$ (*what is x?*) as $F_q . F_x$ in which F_x may be a cypher or a substantive flash.

If we proceed on such lines we can lay down assent or denial by preposition of the radical F_q to a false in juxtaposition to a true statement and postposition of appropriate flashes, viz.: (i) the radicle F_r if the rule as stated is true; (ii) the radicle F_n if the rule is false. The audience may fill in the alternative conjunction (F_o) below. In doing all this we have forged the essential components for two other

A SECOND LESSON IN CELESTIAL SYNTAX

Doubt (F_q) *and Alternative* (F_o)

$F_q..1..F_a..1.1..F_a..1.1.1..F_b..F_s..F_o..F_q..1..F_a..1.1.1..F_a..1.1.1..F_b..F_s$

Confirmation (cf. Welsh)

$F_r..1..F_a..1.1..F_a..1.1.1..F_b..F_s$

Denial

$N_r..1..F_a..1.1.1..F_a..1.1.1..F_b..F_s$

NOTE: In Welsh, the usual method of confirmation is not by the use of an affirmative particle (*yes*), but by repetition of the affirmative content. (Oes heddwch? Heddwch: *Is it peace? yes.*)

syntactical devices: (a) *alternative* statement; (b) *conditional* statement. Within the framework of our arithmetic course we can also add two others for which we ourselves use the ideograms ∴ and ∵. Thus our basic syntax permits us now to distinguish:

(i) affirmation, negation and interrogation,
(ii) assent, denial and doubt,
(iii) conditional and alternative assertion,
(iv) causal and consequential relationship,
(v) *it–they* and *we–you.*

Should we find our Martian pupils backward in home background knowledge of astronomy, some of the preceding speculations about the layout of an interplanetary news-service will be of little relevance to our hopes or fears. Still, we have kept an alterna-

tive in reserve. Since keeping pupils at play because backward and keeping them at play to make them backward are both now accepted targets of educational practice, it would be unfitting to provide our school with no recess or half-day off for play. The syntax which emerges from our first course in arithmetic will make it possible to lay down rules of celestial chess and the invitation to play it. We may then be able to divert some of the deplorable combativeness of our own species by recording interplanetary tournaments to keep the international news out of the headlines.

Our language will remain within the framework of our presuppositions a medium of *collective* intercourse, our celestial culture confined to astronomy, chemistry, physics, geodesy, geology and gamesmanship, unless we can either introduce the concept of *personality* into its vocabulary or make space travel facilities for visitors to penetrate the iron curtain of the ionosphere. As a biologist, I should dislike to relinquish the possibility of knowing something about our Martians as individual beings, and I am not of the heroic company who would lightly volunteer for such voyages into the unknown, unless the space colloquium of the foregoing fantasy had already convinced me of a warm welcome awaiting me. Accordingly, I shall now try to indicate a possible trek to an interplanetary definition of the *ego* through the jungle of celestial demography (below and p. 135).

We recall that we can establish the bipolar concept *we–you* by juxtaposition and repetition of already received and transmitted signals, as soon as we have solved the problem of identifying the interplanetary interrogative particle (F_q). Let us now suppose that we can each detect as such signals from different transmitting stations. We can then use our new question and answer technique to give the radical component F_w (we) of $F_{w \cdot e}$ (*we of the earth*) and $F_{w \cdot m}$ (*we of Mars*) or the radicle component F_y (*you*) of $F_{y \cdot e}$ and $F_{y \cdot m}$ a new contextual reference by association with appropriate qualifiers, e.g. $F_{w \cdot a}$ (*we of site A*) or $F_{y \cdot b}$ (*you of site B*). Given this differentiation, we are ready to lay the foundations of celestial demography by associating with $F_{w \cdot e}$ (*all of us on earth*) $F_{w \cdot a}$, $F_{w \cdot b}$, etc., the compound interrogative particle *how many* ($F_{q \cdot n}$).

FIRST RADIO LEXICON (glyphs only)

$F_a +$; $F_b =$; $F_c 6$; F_x any natural number

$F_{r \cdot s}$ this is the *right* rule for dimension *s*; $F_{n \cdot d}$ this is *not* the right rule for dimension *s*

F_r Yes; F_n No

$F_{q \cdot m}$ What is the missing *m*th term? F_q what? who? which?

$F_{w \cdot e}$ we on earth; $F_{w \cdot m}$ we on Mars

$F_{y \cdot m}$ you on Mars; $F_{y \cdot e}$ you on earth

$F_{w \cdot a}$ we of transmission site A; $F_{y \cdot b}$ you of transmission site B

F_w we; F_y you

$F_{q \cdot x} .. F_{y \cdot a}$ how many of you at site A?

$F_x .. F_e .. F_b ..$ the number of *individuals* is .....

F_e the *Ego*

So we have reached the point at which we are ready to talk about ourselves (*I*) individually, or to ask questions about individual Martians (*Thou*). No new syntactical issues now arise in connection with our first exchange of information about interplanetary anatomy, e.g. what receptors and/or instruments we use, how big we are; what elements dictate the rhythm of life (*I*) and death (*the end of me*)? If the foregoing, and needless to say, very highly speculative premises are admissible, it requires little imaginative effort to prospect the possibility of asking other questions about our Martians or of answering theirs about ourselves. To some extent, a prerequisite of the solution of our biological problem is a common abstract geometry of *shape*. I do not think this will offer a serious challenge to our co-planetary higher mathematicians to whom the term geometry now seems to signify any branch of mathematics if non-visualizable.

Should reciprocal communication develop in some such way as outlined in the foregoing remarks, it would have several consequences beneficial to ourselves. We should have to adopt an interplanetary system of weights and measures (time, mass and possibly temperature), an interplanetary numeral system and a radical revision of algebraic conventions. When we reflect on the stupendous waste of human effort and the frustration of human ability incident to archaic units of measurement, a defective system of enumeration and the all too little recognized chaos of contemporary algebraic symbolism, the speculations in which I have indulged will do the State some service, if they quicken our awareness of one of the truly most challenging problems of large-scale education.

9

THE NEW AUTHORITARIANISM[1]

THE rival claims of authority and of intellectual initiative constitute an antinomy men and women have had to face, and as far as we can foresee will still have to face, at all times and in all places. It is one from which reflective persons can escape neither into the golden age of an irretraceable past nor into a millennium in the unattainable future. To seek an escape from it in either direction is indeed an infantile attitude. Being adults, we must accept it as a perennial responsibility of maturity, examining its implications in the context of our own day and generation.

While controversy over the repeal of the University Test Acts was bitter, when the following of Moncure Conway had not as yet found an honourable niche as a law-abiding branch of British Non-conformity, revival of evolutionary speculation exacerbated an antagonism for which the record of Catholic dogma could offer spectacular precedents. In this setting, the man of science stood out as the custodian of intellectual freedom against the Christian Churches as the bulwark of authoritarian thought; but we shall not be false to the one if we ask whether this is still true, and less than fair to the other if unwilling to entertain the contrary. For the problems of Darwin's time are not those of our own. If the leaders of the Protestant Churches now disclaim the role of their predecessors in the controversies provoked by the publication of the *Origin of Species*, we ourselves have little to lose by acknowledging that the intellectual climate of the mid-nineteenth century was not propitious to a just appraisal of the formative role of early Christianity in the

[1] Condensed from *The Moncure Conway Memorial Lecture* 1949, published by Watts & Co.

social background of modern science. With the disputes of Darwin's time behind us the modern Rationalist can approach the tasks of today without fruitless recriminations, and concede with a good grace how generously the ideology of Christian Compassion contributed to a *milieu* favourable to the advance of medicine and even to that of the mechanical sciences, hitherto imprisoned in the strait jacket of a chattel-slave economy.

Doubtless the Church is still a powerful obstacle to the advance of natural knowledge in Catholic countries; but it is scarcely true to say the same of Protestant communities, where physicists and liberal theologians, in friendly competition with the psychiatrist, chemists and classical scholars infected with the virus of anthropological research, have long since passed one another travelling in opposite directions. The conflict between authority and curiosity remains with us as ever; but our site of operations will be a new one if we are fit to defend the heritage of free men worthy of the example of those who formerly established in these islands the right to know, to utter, and to argue freely according to conscience.

In Britain and in the United States no arbitrary *external* authority directly threatens the right of the individual to revise his beliefs in the pursuit of the scientific method; and if external circumstances indirectly influence his inclination to do so, it is because they conspire with forces operative within the profession of scientific work, more especially in virtue of the sheer bulk of such work in the world today. The temper of large-scale enterprise is, and perhaps necessarily so, authoritarian. For what controls the work of the individual is necessarily remote; and remote control does not readily accommodate a two-way traffic of motivation. Scientific work is not exceptional in this respect. It will increasingly reflect the authoritarian temper of other large-scale undertakings, and in the end relinquish its capacity for further growth, unless scientific workers set themselves to create conditions more favourable to the cultivation of a tolerance towards the conscientious objector to militarism in the intellectual domain.

That size alone sets a limit to the efficiency, and in the long run to the survival, of any pattern of structural organization is a commonplace of biological instruction documented by the authors of the *Science of Life* with picturesque examples, doubtless familiar to many of this audience. Thus the existence of the dragon fly and of the

larger hawk moths, of the flying fox and of the albatross, each in its own way a marvel of the evolutionary pageant, signalizes the climax of a process in which the demands of sheer bulk compete with the surface available for efficient respiration and attachment of the wing muscles. Indeed, a bird can attain the stature of the extinct New Zealand Moas only by forfeiting its prerogative of flight; and the fossil record of Vertebrate life discloses what penalties the dimensions of the anatomical ground plan may exact. For size alone suffices to explain why the Dinosaurs could not meet the demands of the climatic changes at the end of the great Chalk epoch, when they made their exit from the stage of history.

If industrial psychologists and Army psychiatrists have lately and usefully focused attention on the dimensional aspect of morale in a working party, it is still true to say that the dimensional viewpoint has not appreciably penetrated social and political thought. We too commonly extol or denigrate the claims of collectivism regardless of whether we are discussing what it implies in the province of Saskatchewan, with a population half the size of Birmingham, or what in the Soviet Union, with a population five times as great as Britain. We too commonly denounce or idealize the consequences of free enterprise with little concern for the need to distinguish between the retinue of Imperial Chemical Industries in 1948 and the factory in Steelhouse Lane, Birmingham, where Roebuck and Garbett started to operate the Lead Chamber process in 1748. If I find myself, as I grow older, more detached from the threadbare controversy between my Socialist friends and their opponents, it is because we are little likely to reap great benefits which might otherwise accrue from a planned economy, and in a setting which discloses the inescapability of still more planning, unless we concentrate far more attention on what remediable evils are attendant on the scale of social organization; that is to say, how far size alone defeats the ostensible advantages of a particular industrial or political structure.[1]

[1] Before the end of the war any citizen of normal intelligence could have seen that one might as well accept the inevitability of a National Health Service in the spirit of St Augustine's invocation of the City of God (see p. 118). By choosing to rest its case on the superannuated slogans of private enterprise, and hypnotized by a picturesque identification of the Radio Doctor with St George delivering Britannia from the maw of the Socialist dragon, the medical profession let slip through its hands the opportunity of putting a temperate, and irresistible, case for greater regional autonomy. The result is sinister concentration of power in Whitehall, destructive to the initiative of a public-health service fostered by the Fabian tradition in Local Government, and a caricature

If it is more especially with this end in view that I here approach the conflict of science and authority in the present social context, I take it as my theme because scientific workers, variously attracted by larger opportunities or repelled by the prospect of greater external control contingent on the utilization of discovery by the modern State, are singularly little disposed to examine what grievances are inherent in the present organization of scientific research on account of the magnitude of its undertakings. In stating this, I wish to disclaim at the outset the implication that large-scale organization is of itself evil, or, if you prefer it, avoidable. The supreme achievements of scientific discovery in our own age have indeed forced on mankind the necessity of some measure of organization on a global scale as a condition of human survival. Thus the sovereignty of the nation-state in matters military spells death to a civilization on the threshold of the age of atomic power. Henceforth we may be partners in the common prosperity which scientific ingenuity can create, or bedfellows in a common grave; and to that extent we have still to learn the technique of co-operation on a larger scale than heretofore. None the less, we have still to learn the perils of making the working party too large within the framework of such co-operation, and especially in the intoxication of vastly expanding expenditure on the pursuit of science.

One peril comes into focus, if we ask by what token naturalists of an earlier generation asserted their pre-eminence in the vindication of intellectual freedom. For the answer is surely an identification of the scientific outlook with refusal to take propositions about external or human nature *on trust*. Such an attitude was more easy to sustain when it was not impossible for a single intellect to survey data relevant to the major generalizations of natural science. Refusal to take one's beliefs on trust then signified little more than a lively curiosity to face facts unfavourable to orthodoxy, and science demanded little more of its votaries in the leisurely tempo of the Victorian period. This is no longer true. The scope of its theoretical content alone signifies that the scientific worker of today does in fact take more and more of his science on trust, and that less because of the

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of proposals Socialists have consistently advocated since the Webbs wrote their notable book on the issue more than thirty-five years ago. Such are the birth throes of a new society when the professional obstetrician has no therapy other than blood-letting for the succour of the patient.

immense accumulation of facts previously unknown than because the logical instruments at our disposal for co-ordinating them are vastly more various and exacting than those which Faraday or Pasteur, Joule or Mendel had at their command.

In their day, mathematical reasoning played a minor role in the advance of science beyond the confines of astronomy and mechanics, and mathematics so applied did not range far afield from the terrain which the Bernoullis had explored. By the turn of the century the influence of Clerk Maxwell on the theoretical foundations of physics extended into chemistry. By the end of the First World War, through physical chemistry, it had begun to penetrate physiology. Between the two world wars statistical methods became a vogue in agricultural, medical, and psychological research, though there had been little encouragement to make the teaching of mathematics more palatable or more accessible to a growing body of intellectual consumers. In school and college alike the teaching of mathematics, if dictated by any contemporary needs, reflected merely the demands of the engineering sciences.

The vogue of statistical methods calls for special comment if only because of the lighthearted enthusiasm with which Dr Cyril Darlington commends them in the preceding lecture of this series. When he assures a lay audience that 'modern statistical methods, largely developed by Englishmen, have transformed our knowledge of how to extract information from numbers,' and that their full use by Government departments 'would demand new activities of explosive violence,' one presumes that he refers to a calculus concerned with the *observable frequency of events*[1] rather than to a *calculus of judgments*, and one which makes no direct appeal to facts about the way in which we do in fact make right or wrong judgements. Since one must dismiss the possibility that so distinguished a colleague would otherwise write so forcefully, one therefore concludes that Dr

[1] The formal similarities of mathematical operations severally invoked to interpret consequences of sampling and to describe the gross behaviour of assemblages of individuals about which we have no knowledge except of their behaviour in bulk should make us the more careful to distinguish between two sorts of hypotheses to which we commonly apply the epithet *statistical*. The consequences of a statistical hypothesis of the latter kind (e.g. the Kinetic hypothesis or Haldane's Genetical Theory of Populations) are such as we can test by appeal to immediate experience. The credentials of a significance test are on a different footing, the very fact that we appeal to its arbitrament being an admission of our lack of information relevant to the completion of the Bayes' balance-sheet.

Darlington has[1] himself traced through a maze of the abstract geometry of *n* dimensions what logical assumptions are inherent in the development of such a calculus by Englishmen and lesser breeds. Unhappily, such is true only of the exceptionally conscientious or exceptionally gifted investigator.

In these days, when even a training course for teachers takes within its scope an outline of factor-analysis, to say that less than one per cent of scientific workers have any clear grasp of the logical credentrials of statistical methods which they invoke would assuredly be an understatement; and, at that, less relevant to the emergence of a new scientific priestcraft if the logicians of science were themselves unanimous, or if those who employ their prescriptions had alternative means of checking their claims. Neither the one nor the other is true, and the fact that this is so is itself a novel situation. With or without a knowledge of its theoretical rationale, the laboratory worker can call to his aid, with a clear conscience, the glass electrode or the spectrometer, because he can, and in practice usually does, calibrate a new physical method by recourse to a known standard; but there is at present no such known standard by which one can calibrate the results of acting on the assumption that the famous postulate of Bayes leads more or less often to correct judgements about the world we live in. As matters stand, there are at least three sharply divergent views about this among experts who are also Englishmen: that of Professor Harold Jeffreys, that of Professor R. A. Fisher, and that of Professor E. S. Pearson, whose approach to the basis of logical inference most appeals to me. The differences involved are not trivial and sometimes lead to controversy of explosive violence. Indeed, they are highly relevant to the credibility of statistical procedures in common use.

In the higher levels of education and investigation, circumstances contributory to a disposition to take more and more on trust with less and less concern for rational grounds of confidence are evident on all sides. The constant pressure of foundations which finance research, diverts the eager curiosity of the young investigator by speeding up publication of results in any field of inquiry with an aura of novelty. The policy of our institutions of higher learning

[1] Does not Sir Richard Gregory assure us, in his Foreword, that the Lecturer 'stands for freedom of scientific thought and expression without control by any authority, as is represented in the spirit of the motto of the Royal Society, *Nullius in verba*'?

penalizes honest effort devoted to exposition of science to students or public alike by allocating academic promotion solely on the basis of mileage covered by repetitive reprints. In universities or Government research stations, heads of departments ostensibly appointed by reason of capability to do original scientific work soon discover that they are paid to be super-salesmen of problems nobody can solve, when they might otherwise be occupied with problems of which they themselves could usefully find a solution. That the temper of science itself in this context becomes increasingly more authoritarian is thus a truism for which we can rightly blame neither Mr Bevan nor Bolshevism. It is an unavoidable penalty of large-scale scientific inquiry, if we fail to gear the tempo of educational technique to the tempo of teamwork in the laboratory.

To say that the educational machinery of our time is utterly inadequate to take advantage of the tempo of advancing scientific discovery on any front is all too pitiably true of a generation so near in time to that of Joseph Priestley, to that of Faraday or to that of Edison; and the pity of it is the complacency we have in common with every pre-existing culture in its decline. The voice of Robert Graves cries out in a wilderness of authoritarian specialities, when he declares:

> 'To know only one thing well is to have a barbaric mind: civilization implies the graceful relation of all varieties of experience to a central humane system of thought. The present age is peculiarly barbaric: introduce, say, a Hebrew scholar to an ichthyologist or an authority on Danish place names and the pair of them would have no single topic in common but the weather or the war (if there happened to be a war in progress, which is usual in this barbaric age).'

This new authoritarianism of the campus and of the laboratory would signify little if we could turn for inspiration to the modern school, where pedagogy has petered out in an orgy of pedophilia. Here the picture is more disturbing. A tradition of self-confident discipline, at least concerned to enforce a minimum standard of culture, if often misdirected, to promote book-learning out of tune with modern requirements, has now made way for a saccharine preoccupation with self-expression inconsistent with the character of a culture steering its own course. The happiness of the child is not

the goal of education in a society confident of its own future. At best it is a technique more or less relevant to accomplishing one or other of the diverse aims of education with a serious end in view. We can arrest a process which ensures more and more people knowing less and less about what others are doing only if we undertake an all-round and intensive speed-up of the educational process at all levels from the cradle to the grave, and with the fullest use of the new Visual Aids at our command.

Otherwise, and inescapably, increasing specialization implies a new authoritarianism within the ranks of the professional scientific worker, and the more so because of external circumstances which scientific workers are themselves reluctant to recognise. The costliness of contemporary scientific research itself has consequences with little direct relevance to the more colourful social controversies, which sustain the romantic ideal of the man of science as the custodian of intellectual freedom, by distracting attention from unpalatable, because less flattering, considerations. There was a time when a man of comparatively modest independent means could devote himself to experimental work, and of the highest calibre, with no patron to rely on. This is no longer so. Increasingly elaborate techniques call for expenditure on a scale which necessitates the support of either the State or of private foundations; and the new patronage imposes a new discipline on the process of discovery, reshaping the outlook of its personnel.

To justify the newer and more costly techniques it is necessary to show results. Accordingly a memoir in a scientific journal is less now, as once in my own lifetime, the announcement of a new discovery than a progress report on a cash advance. Between the two wars there has been an unprecedented proliferation of specialist journals; and the rising tempo of scientific publication now confronts the recruit to science in the age of minute-conscious man with a dilemma destructive to the delusion that a career in science is a life of intellectual adventure. Either one plays for safety, and in so doing risks the loss of inclination to accomplish; or incurs the danger of falling behind in the paper-race by following a promising clue, and in so doing forfeits opportunities of exploring an alternative. To play safe in the new setting itself signifies two things: to exploit to the fullest extent a technique the mastery of which means indispensability to a team of others, each with his own technical speciality,

and to sift gargantuan bibliographies for indications of a new means of doing so regardless of interest in the solution of a problem on its own account. In this situation, it is a thankless undertaking to re-examine the theoretical foundations of one's particular discipline, and well-nigh impossible to sustain an unequal struggle with the contemporary output of publication in order to view the advance of science on a wide front in the discharge of what teaching is contingent on scientific employment.

If this view is admittedly and advisedly gloomy, let me say that I see no sufficient reason for excessive pessimism of the outcome other than the vehemence with which many of my colleagues would disclaim some of the foregoing assertions. Reasons for reluctance to acknowledge the outcome of forces which have shaped the destiny of the scientific worker in the interwar years are not far to seek. One is that to do so is to renounce an agreeably romantic ideal of the man of science as the knight-errant of intellectual freedom against the combined forces of Church and State; and an antimony inherent in all workmanship of high quality aggravates the difficulty of relinquishing this comfortable fiction. To sustain the morale of a team it is necessary to invest its work with a sense of disproportionate urgency; but such auto-suggestion is inconsistent with the reflective reserve which holds authority in check.

Other circumstances help to explain our disinclination to recognize how largely the large-scale character of contemporary scientific endeavour conspires to reinstate authoritarian habits of thought within the citadel of science itself. Till the outbreak of the late war the leaders of team-work in science, men such as Rutherford and Millikan, Morgan, and Sherrington, had reached their eminence within the framework of an earlier tradition. Some of them were great teachers; and in America, where expenditure on team-work was lavish during the interwar decades, the prospect of pressure for quick returns could occasion little dismay among those eager to take advantage of a new battery of techniques so lately available because of the commercial development of radio. Those who will now fill their places are of a different vintage; and we have still to see the impact on science of a new generation which has tasted power fortified by secrecy in wartime operational research, exposed to insidious temptations satirized in Nigel Balchin's *Small Back Room.*

To say this implies no allocation of blame. High-level Civil Servants trained in a school disdainful of science, harassed by the uncertainties of a situation for which classical lore furnished no precedent when the magnetic mine threatened our mercantile marine with annihilation, were eager enough to treat men of science as magicians, if by so doing they could transfer responsibility of disaster to other shoulders. In this mood the intrepidity of a middle generation with a future before it was more congenial than the reticence of older men with nothing to lose from the admission that scientific experts are sometimes fallible and do not always have a ready-made recipe for any contingency. By and large, the middle generation proved more than equal to the immediate assignment, as the outcome has shown; but men of science would not be men at all if so sudden an investiture of their work with well-nigh supernatural veneration and unaccustomed privilege left no impress on their personalities, be it ever so little.

Henceforth and ineluctably in Britain, which is now a have-not community, the scientific leader must be somewhat of a showman to enlist the goodwill of reluctant patrons. Because his patrons are administrators, men earmarked for early promotion will be men with the administrative viewpoint; and the administrative is necessarily a trustful, hence authoritarian, type, being less concerned with how we come to know than with what we acceptably claim to know. In some ways they will be men of wider humanity than their predecessors, for they will travel more in a vicious cycle of international conferences, always with an ear to the ground in a perpetual search for novelty. Intelligent anticipation of the rewards of exploiting an as yet unconsidered trifle picked up in Prague or Pittsburgh will count for more with their patrons than genuine originality, and a modicum of contra-suggestibility, so essential an ingredient of intellectual honesty, will be a luxury for which their patrons will not pay hard cash.

If we ask what manner of man will be the future leader of science in our own impoverished country, we should therefore take stock of the fact that he will have a new ally in the Press. Between his predecessors and the Press there was deep-seated suspicion; but that is a thing of the past. Editors have discovered that science is saleable if invested with the glamour of the screen in suitable captions which announce every new Hollywood film as a breath-taking,

soul-stirring masterpiece. So the future leaders of science will be able to luxuriate in the felicitous assurance of promoting a public interest in science, if they hit the headlines in a build-up which will bring their activities before the notice of those able to dispense large appropriations for research. In doing so, they will easily convince themselves that they are taking upon themselves the mantle of Faraday and T. H. Huxley, whose lectures to working men are in fact masterpieces of lucid exposition addressed in the tradition of the Protestant West to those with an inclination to seek reasonable grounds for beliefs which the contemporary culture prescribes; but the truth is that no scientific correspondent of a newspaper can hold down his job unless he can serve up his copy in the glorious technicolour of mystery acceptable to an appetite for up-to-the-minute sensation promoted by the film industry.

This congenial alliance will generate its own scale of values. If the system of external patronage calls for leaders who know how to use publicity judiciously, and with the sincere conviction that they are serving science in doing so, the system of internal patronage will adjust rewards for service to science, and with equal sincerity of motive, to confer on those with what personal qualities the situation calls for a prestige commensurate with the power they exercise over their younger colleagues. In short, pure mathematics, which is the only surviving ancillary of the natural sciences able to accommodate the eccentric, will soon be the only field in which conformity to authority, a shrewd eye for novelty, and an accommodating committee-technique of man-management will count for less than a craving for intellectual adventure regardless of social advancement.

If such are real dangers in the trend of large-scale organization of research, those who do recognize them offer little prospect of remedial action by extolling the search for truth as a Mandarin virtue or by blaming the State, which has often been a hard taskmaster both by demanding quick returns and by restricting freedom of publication. Those who do so wish to have their cake and to eat it. For modern research is costly, and the opportunity to pursue it is conditional on some accommodation with those who can endow it. Whether the fortune of Lord Nuffield passes through the sieve of H.M. Inland Revenue *en route* to the laboratory, or reaches the same goal through the vicarious munificance of Mr Farrar Brown, the results are much the same—at least as they affect the outlook of the

scientific worker. He who pays the piper calls the tune. No State department could be more arbitrary in its appropriations than the Nuffield Foundation, which has undertaken to vindicate the professional exploits of its beneficiaries by printing as brochures what accredited specialist journals have no opportunity to refuse.

In the competitive struggle for endowments, without which the scientific worker is powerless to carry on, there is evidently much to corrupt men and women drawn to a career of science by disinterested curiosity. It is therefore right that scientific workers should protect themselves from the rigours of an intellectual climate of apprehension equally devastating whether it entails subordination of the search for truth to the whims of motor magnates or to the dictates of a bureaucracy; but the battle cry of *knowledge for its own sake*, coupled with comminatory deprecation of any interest in the social background or in the humane content of science as a Marxist deviation, does not resolve our dilemma. The tag itself invites citation of a much-quoted rejoinder of Samuel Butler. When asked to disclose his attitude on art for art's sake, he put the question: 'What is art to have a sake?' The problem of accommodating security without subjection to arbitrary authority is not a problem peculiar to scientific work alone. It is a problem of the century of the common man.

Knowledge for its own sake presumably means knowledge for the sake of the individual, the pursuit of knowledge for the satisfaction of personal curiosity; and it goes without saying that individual curiosity is a necessary condition for the growth of knowledge. To that extent, knowledge for its own sake is a formula which draws attention to a legitimate aspiration and to a sound educational principle. Most of us, including those of us who are interested in historical circumstances propitious to the expansion of human knowledge at different times and in different places, would deplore restriction of inquiry to topics which offer only immediate practical results. Most of us, including those of us who regard education as an instrument for civic enlightenment, would also agree that good teaching is only possible if the teacher is intensely interested in the topic of instruction. Most of us, including those of us alert to social agencies which provide scope for personal curiosity of one kind or another, would readily concede that personal interest in a particular line of inquiry or instruction depends on circumstances which do

not necessarily or commonly have a close connection with its social usefulness.

To make due allowance for the desirability of encouraging research into topics which offer little immediate prospect of practical application, as also for the circumstance that personal interest with no direct relation to the social value of a topic is a valuable and essential part of equipment for investigation or teaching alike, does not exclude the possibility of recognizing the growth and diffusion of knowledge as an essentially social phenomenon. At any given moment in history, the contribution of an individual, however gifted, is small compared with the sum of our social inheritance. At any given moment in history, the influence of the individual, however gifted, depends on prevalent social values and available social amenities. Historical experience leads some of us to believe that the fullest practical use of scientific discovery *en rapport* with a vigorous policy of popular education encourages confidence in the scientific venture, and thus ensures greater freedom to explore themes which offer no quick return for the effort spent. As an historical proposition this view may or may not be as right as I believe it to be. Right or wrong, it is not in opposition to any reasonable profession implicit in the plea for the pursuit of knowledge for its own sake. On the contrary, it is a recipe, good or bad, for promoting what reasonable claims knowledge for its own sake implies.

Few of us who hold the view I have just stated imagine that precocious concern for the remediable evils of malnutrition mainly determines the choice of a career in biochemistry. What we do assert is that a society more alive to the problems of malnutrition would furnish more ample opportunities for biochemical research and instruction. We believe that more widespread knowledge of the *uses* of biochemistry would encourage more sympathy for the work of the people who are genuinely curious about the chemical economy of living creatures. We find it difficult to understand why biochemistry should suffer from the efforts of those who wish to promote greater public esteem for the work which biochemistry carries out; and we are not convinced that a person who professes supercilious indifference towards the welfare of his fellow citizens is on that account a better biochemist.

If it is still apposite to expose a false antithesis which has so long clouded discussion of the place of science in the modern world, it is

relevant to my main theme, because it is the focus of a controversy which distracts the contestants from an appraisal of the consequences of the contemporary scale of expenditure on their own researches. In this disputation, contemporary scientific workers fall into two camps, the Mandarins and the Fellow Travellers. The Mandarins resent the claims of society on themselves and affect an aloofness no longer consistent with their own claims upon society. In their enthusiasm for the new security and new prestige a planned economy can confer on the profession of scientific research, the Fellow Travellers are impatient with those who recognize the need to cherish an intellectual tradition consonant with the fullest exploitation of greater material opportunities and prerequisite to a healthy relation between theory and practice. I suggest that such a tradition is inconsistent with the habit of taking on trust what we have no means of checking by recourse to experience, that the present tempo of scientific publication within the present framework of education encourages the habit of doing so, and that the conflict between science and society is therefore a less profitable theme than the conflicting motives of the scientific worker himself (or herself) in this *milieu*.

This internal conflict between the inherent curiosity of the scientific attitude and the opportunism which the conditions of our time increasingly prescribe as a condition of scientific work must needs resolve itself in fact by a supreme effort we are loth to undertake, or in fantasy by shifting the guilt to an external authority whose elimination will supposedly effect a remedy for an imperfectly formulated grievance. This helps to explain why creeds such as racialism and Communism, inviting violent and destructive measures, have a peculiar appeal to scientific workers who realize, if through a glass darkly, some of the implications of living in society; but another characteristic of their work and training conspires to aggravate this disposition to impatience. While it is now the hallmark of good instruction in the natural sciences to place emphasis on comprehensive generalization in contradistinction to fact-finding, and especially since physical chemistry and experimental biology have co-ordinated so much of the content of nineteenth-century natural history, men of science with no background of humanistic studies—ancient or modern—are especially unaware how much good judgement in social affairs still owes to a lively knowledge of

the natural history of man himself and into how many pitfalls facility in extensive abstraction can lead the explorer in an unfamiliar terrain.

In the *Making of Humanity* Robert Briffault distinguishes between two intellectual attitudes inimical to the fullest exercise of man's reason, the one he calls *custom-thought*, the other *power-thought*. My predecessor in this series of lectures identifies the chief obstacles to the growth of science with the custom-thought of our seats of learning and the power-thought of our rulers. As I interpret his plea, his remedy for the one is greater emphasis on research in the practice of teaching, and for the other a transference of political authority to a scientific *élite*.[1] For my own part, I think that the most powerful obstacles of custom-thought and power-thought to the healthy growth of science are inherent in the present organization of research itself. I believe that the remedy for the one or the other is a drastic reorientation of teaching with more reflective emphasis on the humane background of natural science than on the novelty of its content. Meanwhile, the prospect of living under a technocracy devoid of the sympathy fostered by a now effete humanistic tradition is not likely to materialize, and happily so.[2]

That I am not alone among my contemporaries in the viewpoint I have advanced will appear from the following remarks of my friend

[1] Dr Darlington's own words (p. 28) are: 'It is against the background of conflict and confusion in the relations of science and society that we find ourselves confronted with a crisis in the history of mankind, and particularly in the history of human government. It is a crisis arising from the rapidly increasing power given to man by science. It is a crisis such as we are accustomed to leave to the arbitrament of sectional interests supported by shouts and cries. But it is one to which scientific inquiry can provide a solution. For *the fundamental problem of government is one that can be treated by exact biological methods. It is the problem of the character and causation of the differences that exist among men, among the races, classes, and individuals which compose mankind.* The little passions and prejudices we have been discussing so far fade into nothingness in face of the gigantic errors and illusions that can be, and are being, mobilized to defeat or pervert scientific truth in this field.' (*Italics mine.*)

We may recall, with profit, remarks of Einstein in a letter to Freud on war: 'How is it possible to control man's mental evolution so as to make him proof against the psychoses of hate and destructiveness? Here I am thinking by no means only of the so-called uncultured masses. Experience proves that it is rather the so-called *Intelligentzia* that is most apt to yield to these disastrous collective suggestions, since the intellectual has no direct contact with life in the raw but encounters it in its easiest synthetic form —the printed page.'

[2] Unless a recrudescence of the Rosenberg-Darlington creed again plunges the world into untold misery by stiffening an impulse to cruelty otherwise inconsistent with human laziness.

H. J. Muller in his speech at the Nobel banquet on the occasion of the award he received as the most outstanding geneticist of our time:

'. . . most of us scientists, I hope, have come to know that, both for the further expansion of human understanding and human control over nature, and also for the increase of our general human well-being—in fact, for the very saving of us from destruction—there are today two great underlying requirements. One is that the thousands-of-years-old growth (oftimes very painful) in the freedom of the human spirit to examine into and question all things, and to communicate one's thoughts, doubts, findings, and criticisms freely in every field, should go on, and should be greatly accelerated, and spread to all the peoples of the earth. This means a society of men educated in all the fundamentals of modern knowledge, and trained to think independently and fearlessly. But to be effective, it means, secondly, the bringing of the benefits of modern techniques to all, the raising of the standard of living of the broad masses everywhere, not exclusive of any groups or races, as they could now be raised with the aid of modern science, so as to make high culture and free criticism not the unstable possession of a limited sector of society, as among the ancient Greeks, but a part of the accepted inheritance of all humanity. This implies among other things not only suitable employment for all but also a more effective, more informed, and more direct participation than exists in most places, on the part of what are now the fourth and fifth estates, in the making of decisions affecting themselves and the commonalty. For human nature has never proved altruistic enough to allow the interests of one group to be successfully entrusted to another one. Necessarily this will require some sacrifice on the part of some of those now more favoured, but it will be to the benefit of their children in the end. History indicates the suppression of either one of these two related modes of progress to be equally disastrous. In fact, the destruction of civilization, and of most of mankind, today, would hardly be more deplorable than the sealing of the bonds of either our intellectual or our economic enslavement by the victory, even if bloodless, of any system which, though it temporarily offered one of these two types of development, gave no hope for progress in the other. . . .'

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